THE EFFECTS OF NONANALYTIC AND ANALYTIC THINKING IN NEW PRODUCT FORECASTING

By

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TABLE OF CONTENTS

Page

ACKNOWLEDGMENTS iii

ABSTRACT vii

CHAPTERS

1 INTRODUCTION 1

New Product Forecasting 2
Analytic versus Nonanalytic Judgment 5
Research Propositions 9
Overview 10

2 SCENARIOS 12

Imagination versus Explanation 13
Scenarios versus Reasons 17
Debiasing Scenarios with Analytic Thought 19

3 PILOT STUDY 1: IMAGINATION AND EXPLANATION 22

Method 23
Results and Discussion 25

4 PILOT STUDY 2: SCENARIOS AND ELABORATION 30

Method 32
Results and Discussion 34

5 STUDY 1: SCENARIO ANALYSIS 41

Multiple Explanation 43
Scenario Analysis 48
Method 51
Results and Discussion 53

6 STUDY 2: SCENARIOS AND ACCOUNTABILITY 63

Method 65
Results and Discussion 66

7 ANALOGIES 72

8 PILOT STUDY 3: ANALOGIES AND ELABORATION 76

Method 78
Results and Discussion 79

9 PILOT STUDY 4: ANALOGY EFFECTS IN A MORE CONSERVATIVE TEST 88

Method 89
Results and Discussion 92

10 STUDY 3: ANALOGIES AND COUNTER-ANALOGIES 104

Method 108
Results and Discussion 110

11 STUDY 3A: ANALOGIES AND COUNTER-ANALOGIES IN A MARKETING SIMULATION ENVIRONMENT 117

Method 118
Results and Discussion 120

12 STUDY 4: ANALOGIES AND DECOMPOSITION 126

Method 129
Results and Discussion 130

13 GENERAL DISCUSSION 138

Stickier Priors 139
Construction in Judgment 144
Other Issues 145
Managerial Implications 149

REFERENCES 153

BIOGRAPHICAL SKETCH 166

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Judgmental forecasting is an inherent component of new product development and strategic decision-making. Managers and firms often struggle with these decisions, as evidenced by failure rates of new products and new businesses. This dissertation investigates judgmental processes that may bias forecasts in a new product development context. For my purposes, judgments are classified as nonanalytic or analytic. Analytic judgments reflect an evenhanded approach based on causal reasoning. Various tools exist to assist analytic judgment (including accountability, scenario analysis, counterfactual reasoning, and task decomposition). In contrast, nonanalytic judgment lacks the depth and evenhandedness of analytic judgment. Examples of nonanalytic thinking include imagining scenarios and reasoning by analogy.

I propose that nonanalytic processes can have powerful effects on subsequent predictions and that the effects of nonanalytic thinking in forecasting situations are fairly resistant to subsequent analytic thought. In addition, the effectiveness of analytic techniques in debiasing judgment may depend on how these biases were initially created. Thus, understanding the judgmental processes involved in nonanalytic and analytic thought and their mediating cognitive processes is critical. Assessing the effectiveness of analytic tools and understanding judgmental processes will provide managers with insight into current and best practice in judgmental forecasting. In addition, these results will also generalize to consumers domains, including how consumers respond to new product concepts, predict their own behavioral intentions, and plan for their own futures.

My research consists of a series of laboratory studies in a (simplified) new product development context. These studies demonstrate systematic effects of nonanalytic thinking on judgment. Scenarios and analogies are shown to have enduring effects on judgment, despite subsequent use of analytic tools. Counterfactual reasoning failed to eliminate scenario and analogy effects on judgment. Stronger debiasing attempts that used counter-scenarios, counter-analogies and decomposition also failed. Instead, some evidence suggests that subsequent efforts to be analytic after nonanalytic thinking exacerbate bias. For example, accountability increased scenario-induced optimism, and the presentation of evenhanded facts may have polarized judgment. Overall, these findings demonstrate the robustness of nonanalytic effects on judgment and raise questions about the effectiveness of various analytic tools discussed in the literature.

CHAPTER 1
INTRODUCTION

Predictions are ubiquitous. They occur among consumers in common everyday situations ('Will this new product really deliver its promised benefits?' 'How often and for how long will I use this product before I need another one?' 'How much will I spend this week/month?'), and also permeate business life ('Will this new product be successful?' 'How will this new market or channel affect my sales?' 'What new technologies will affect my business, and how?' 'Will this strategic alliance benefit my firm?'). Accurate predictions are usually functional, because they enable people to adapt to future events and sometimes to intervene and change the future.

Most everyday predictions involve human judgment. Interestingly, surveys suggest that the most popular business forecasting methods are based on intuitive judgment, including the sales force composite, jury of executive opinion, and naïve extrapolation (Armstrong, Brodie, & McIntyre 1987; Dalrymple 1987). However, most research efforts in marketing have focused on developing and refining new quantitative forecasting methods, and have neglected intuitive methods that rely on human judgment. This research attempts to redress that imbalance by investigating the human judgment processes that underlie forecasting in business. In particular, this research focuses on judgmental forecasting in order to answer the following questions: How do people make predictions? Are these predictions biased? If so, how do they become biased? Can people correct for these biases? If so, how do debiasing techniques work and do they vary as a function of the cause of the original bias?

New Product Forecasting

Judgmental forecasting may be used in a variety of managerial and consumer contexts. From a managerial perspective, entrepreneurs and new product managers may rely on intuitive judgment to forecast new business or new product success and to develop strategic initiatives and business plans for an uncertain future. From a consumer perspective, judgmental forecasts may be used in personal financial and career planning and in consumer budgeting and time management. This research focuses on the domain of new product innovation, an increasingly important driver of sales and growth for Product Development and Management Association (PDMA) firms (Rosenau 1996). New product forecasting can be investigated from both a consumer perspective (e.g., judging new products in concept testing and focus groups, and in buying situations) and managerial perspective (e.g., screening new product ideas, forecasting the success of new products and/or the impact of new technologies).

New product forecasting provides an interesting opportunity to examine managerial forecasting, especially in the early stages of new product development that lack 'hard data' upon which to base predictions. Indeed, the new product idea mortality curve for PDMA member firms indicates that about 70% of new product ideas are screened out before reaching the testing or launch stages (Rosenau 1996).

The new product context has several important characteristics (also shared by the other domains identified above) that may increase reliance on judgmental forecasting. First, new products tend to be viewed as relatively unique, so historical data on other new products may be neglected. Second, new product forecasting is a complex task and managers may prefer holistic evaluation of a single identified new product alternative rather than an evaluation across the (potentially infinite) set of other alternatives. Both of these characteristics may favor judgmental rather than quantitative methods and nonanalytic rather than analytic processes. Third, new product concepts may shift from relatively abstract ambiguous ideas in the very early stages to relatively specific prototypes in the later stages of development. In conjunction, judgmental processes may also shift. Fourth, managers may become quite personally involved in new product development projects, which may heighten biases due to desirability and controllability. Fifth, the time horizon and other criteria against which to predict new product success may be variable or ambiguous, which exacerbates difficulties in predicting short- and long-term consequences in analytic judgment. And, finally, the multitude of factors that influence new product launches (e.g., the product itself, its marketing mix, the marketplace, and the competition) may make learning from new product experiences quite difficult for firms and managers. This difficulty in learning analytic models of new product success may favor a more holistic approach to forecasting.

New product development is also an interesting forecasting domain because of the documented evidence of inaccuracies in forecasts. Anecdotal evidence suggests that many acclaimed experts in technology and science, business, and the arts have erred considerably in their predictions (see Table 1). Anecdotal evidence also points to the use of a wide range of judgmental processes--from relatively analytic to nonanalytic--in new product forecasting. One example of remarkably accurate forecasting based on relatively analytic judgment is Moore's Law. Gordon E. Moore, co-founder of Intel, extrapolated the growth in the number of transistors on chips to predict that it would continue to double every year for the next 10 years; he also reasoned that falling costs would lead to an explosion in chip applications (Business Week, 23 Jun 1997). However, analytic judgment may also lead managers astray. For example, Coca-Cola may have relied too much on market research data regarding taste preferences in forecasting the success of New Coke (Hartley 1995).

There is also evidence of nonanalytic judgments in new product forecasting. For example, Disney executives overestimated revenue and profit for Euro Disney as a result of relying on an analogy to Tokyo Disneyland (Hartley 1995). Information technology development and forecasting also relies heavily on analogies, most recently to ant colonies and biological organisms, both to understand future systems and to predict the direction of their development (Business Week, 23 Jun 1997). In addition, the role of scenarios in new product development and forecasting seems to be growing in acceptance, from Disney's "imagineering" to Motorola's "vision" of a wireless future (Hamel & Prahalad 1994) to scenario analysis techniques (e.g., Wack 1985; Ringland, Todd, & Schwartz 1998). As these examples suggest, new product forecasting is one area of marketing that may benefit or suffer from divergent approaches to forecasting. Certainly, there appears to be room for improvement in managerial judgment. An estimated 45% of new products fail after launch (Rosenau 1996), and less than 50% of new businesses survive over 5 years (Cooper, Woo, & Dunkelberg 1988). Why is this the case, and how can these judgmental forecasts be improved?

Analytic Versus Nonanalytic Judgment

The goal of this dissertation is to investigate the effects of analytic and nonanalytic thought on judgment within a forecasting context. For present purposes, predictions are defined as judgments of future events. Events can refer to objects, persons, situations, behaviors, or variables that are the target of prediction. These first-order judgments can include qualitative predictions of future event states (e.g., new product success/failure), point estimates of quantities (e.g., new product sales), or point estimates of probabilities (e.g., subjective probability of success of a new product). These first-order judgments can be qualified by a second-order judgment of confidence in qualitative predictions (e.g., probability that the prediction is correct) or in quantitative predictions (e.g., 95% confidence interval for a quantity or probability).[1] I am primarily interested in these first-order predictions, although I also will investigate second-order judgments of confidence.

I classify judgmental business forecasts into two broad categories: analytic and nonanalytic judgments. *Analytic judgments* reflect people's efforts to apply reasoning and logic in a relatively objective and evenhanded manner to arrive at a prediction. In addition, people can use reasoning to consider evidence ('pros and cons') and causal relationships (based on a mental model of causes and effects) to arrive at forecasts. For example, people can estimate new product success by considering base rates for product success or trends in historical sales data or by identifying and analyzing the causes of success or failure of a new product. Various tools have been proposed in the literature to assist analytic judgment (e.g., scenario analysis, counterfactual reasoning, decompositional methods of judgment), and analytic thought is also thought to be enhanced under certain conditions (e.g., incentives for accuracy, accountability, learning through feedback).

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[1] The existing literature is somewhat confusing regarding this terminology. I have adapted an approach similar to Peterson and Pitz (1986).

Nonanalytic judgments are comparatively less 'reasonable.' People may apply reasoning in a one-sided and subjective manner, drawing on evidence or analogies. In addition, people may use mental simulation or imagery-based processing to imagine future scenarios ('mental pictures' of events leading to a future end-state or of the end-state itself) and arrive at a prediction. For example, people may try to imagine or envision the success of a new product in the marketplace (such as consumers successfully using the product, the marketplace conditions under which a product would succeed, and/or a scenario of events leading to a successful new product launch). I surmise that nonanalytic thinking (including scenarios and analogies) may be relatively common and even encouraged in some forecasting contexts.

This distinction between analytic and nonanalytic judgment is reflected in prior research. Alba and Hutchinson (1987) discuss analytic and nonanalytic processes in search, categorization, and inference-making. "Degree of analysis refers to the extent to which consumers access all and only the information that is relevant and/or important for a particular task" (p.417). In their view, analytic processing leads to superior judgment whereas nonanalytic processing is biased by the relative accessibility of information. For example, similarity-based inferences (which form the basis of reasoning by analogy) are considered nonanalytic because similarity is based on salient rather than causal features. In contrast, schema-based inferences are considered analytic inasmuch as they are driven by a causal schema and reasoning about relevant attributes. In general, analytic processing is effortful because it goes beyond accessible information and ignores or discounts irrelevant information; hence, people must be motivated to engage in analytic processing. Hutchinson and Alba (1991) provide evidence that analytic processing is reduced by higher memory loads, incidental (versus intentional) learning, and increased salience of non-criterial attributes. Nonanalytic subjects relied heavily on salient attributes and processed 'pseudo-analytically' by attending to a subset of available information.[2]

Some researchers propose that human judgment is characterized by two separate systems of thought based on this distinction. In particular, Sloman (1996) makes a case for two systems of reasoning. In the associative system, processing is driven by similarity and correlations, and judgments are reached when associations settle into a stable state that maximally satisfies parallel constraints (coherence). In contrast, the rule-based system "describe(s) the world by capturing different kinds of structure, structure that is logical, hierarchical, and causal-mechanical" (Sloman 1996, p.6). It is driven by rules that people understand and apply at either an abstract or pragmatic schema level in order to support explanations.[3] According to Sloman (1996), the two systems of thought are complementary. The rule-based system specializes in deliberate analysis of relevant features whereas the associative system follows connections without logical filtering. Sloman and others note the potential for conflict in everyday reasoning and choice behavior. A rule-based structure provides a more compelling justification for a response than does an associative one, but it is difficult to ignore associative responses.

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[2] These conclusions are reminiscent of those based on dual process models of social judgment, including the elaboration likelihood and systematic-heuristic models of persuasion (for a review, see Eagly & Chaiken 1993). In these models, one judgment route is argument-based and analytic; the other route is less analytic, influenced by peripheral cues and heuristics. Smith and DeCoster (1999) and Epstein and Pacini (1999), among others, provide useful reviews of evidence for dual process models.

As will be demonstrated in this dissertation, predictions can be based on analytic and nonanalytic judgment processes. In particular, I propose that nonanalytic processes can have powerful effects on subsequent predictions and that the effects of nonanalytic thinking in forecasting situations are fairly resistant to subsequent analytic thought. Thus, the judgmental process used to reach a prediction will be shown to have consequences in terms of susceptibility to errors in forecasting accuracy. Moreover, nonanalytic processes may inhibit the effectiveness of various corrective analytic techniques.

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[3] In a similar vein, Allen and Brooks (1991) distinguish between analytic and nonanalytic cognition based on whether the processing is based on a stored abstraction (prototype or rule) or an instance or exemplar. The key difference between Allen and Brooks (1991) and Sloman (1996) lies in their views of prototypes. Allen and Brooks view prototypes as abstractions (like rules), leading to analytic processing; Sloman views prototypes as exemplars, leading to nonanalytic processing. The distinction is not critical to the present research.

Research Propositions

Two types of nonanalytic influences on forecasting are the focus of this research: scenarios and analogies. To understand their influence on judgment (and the overarching rationale for this research), consider what is likely to happen if the manager is optimistic. I propose that imagining scenarios and reasoning by analogy are forms of elaboration that are guided by a focal hypothesis. For a plausible product idea, the hypothesis tends to be one of success, else the product idea would not be considered seriously by the manager. Thus, managers tend to imagine scenarios describing success and reason by analogy to past product successes. Such thought is unduly influenced by associations that are causally irrelevant to new product success (e.g., mental imagery in scenarios, superficial similarities in analogies). In addition, the focal hypothesis inhibits consideration of alternative hypotheses; such mental sets are difficult to break and lead to confirmation bias and belief perseverance. As elaboration continues, a schema develops that supports the focal hypothesis and drives subsequent thinking. The schema drives inference making and leads to a well-integrated set of beliefs and inferences. A scenario- or analogy-driven schema that is relatively coherent and consistent tends to be unduly convincing and influential; that is, the whole is greater than the sum of its parts. This enriched schema increases the availability and perceived plausibility of the focal hypothesis, thereby influencing subsequent judgment. The source of bias due to nonanalytic thinking lies in the one-sided nature of scenario-driven and analogy-driven thought. This one-sided elaboration is a source of mental contamination that is difficult to undo. People are often unaware of and unable to correct for scenario- or analogy-induced effects on judgment. Thus, the effects of nonanalytic thinking persist despite efforts to debias judgment using various analytic tools.

This discussion leads to two basic propositions underlying this research:

P1. Nonanalytic thinking about scenarios and analogies has powerful effects on judgment.

P2. The effects of nonanalytic thinking are resistant to subsequent analytic thought.

Overview

For ease of exposition, this dissertation is divided into two parts. In the first part, I examine scenarios and provide experimental evidence supporting the basic propositions; in the second part, I examine and provide experimental evidence for analogies. Each part of the paper will begin with an overview of prior literature related to nonanalytic thinking (scenarios and analogies). This review will serve as background to support Proposition 1 while at the same time demonstrating the paucity of evidence for Proposition 2.

Each set of studies is designed to investigate the effects of nonanalytic thinking in a new product forecasting context. The majority of studies are conducted in a controlled laboratory environment using experimental research techniques. The laboratory studies are designed to reflect (simplified) new product development settings. In a typical study, subjects play the role of a marketing manager asked to forecast new product success and sales, given a description of a new product concept (e.g., electric cars, electronic books, Internet shopping). Subjects are instructed to engage in nonanalytic thought (e.g., imagining scenarios, reasoning by analogy) prior to forecasting with the aid of various analytic or debiasing tools (e.g., counterfactual reasoning, accountability, decomposition, counter-scenarios, counter-analogies). The effectiveness of these analytic tools is the focus of the investigation. Thus, each study is designed to provide evidence for specific hypotheses related to both research propositions.

Table 1.
Judgmental forecasting errors.

"There is no reason for any individual to have a computer in their home." (Ken Olson, President of Digital Equipment Corporation, 1977)
"With over 50 foreign cars already on sale here, the Japanese auto industry isn't likely to carve out a big slice of the U.S. market for itself." (Business Week, 2 Aug 68)
"My imagination refuses to see any sort of submarine doing anything but suffocate its crew and founder at sea." (H.G. Wells, 1902)
"In…Wuthering Heights…all the faults of Jane Eyre are magnified a thousand fold, and the only consolation which we have in reflecting upon [it] is that [it] will never be generally read." (James Lorimer, North British Review, Aug 1849)
"We don't like their sound. Groups of guitars are on the way out." (Decca Recording Company executive, turning down the Beatles, 1962)
"That's an amazing invention, but who would ever want to use one of them?" (Rutherford B. Hayes, U.S. President, 1876, after trying the telephone)

Source: Cerf and Navasky (1984).

CHAPTER 2
SCENARIOS

The first part of this dissertation focuses on scenarios. For present purposes, a scenario is defined as a story that describes how events unfold over time and includes vivid details that create a mental picture or vision of the future. Scenario generation appears to be an exercise of the imagination as people form a mental picture or vision of the future. Concrete details typically included in a scenario make it seem more plausible and convincing. Such details may be based on associations and mental imagery that are not necessarily causally relevant to the future. Scenarios describe the future by linking these elements together in story-like form.

Prior research is consistent with this view of scenarios. My definition is very close to that of Russo and Schoemaker (1992), who describe scenarios as "script-like narratives that paint in vivid detail how the future might unfold in one or another direction" (p.13). Kahneman and Tversky (1982) propose that people run mental simulations to construct scenarios or stories in order to make predictions or judgments under uncertainty. "A good scenario is one that bridges the gap between the initial state and the target event by a series of intermediate events, with a general downhill trend and no significant uphill moves along the way" (p.207). According to the authors' simulation heuristic, the ease of scenario construction and the "goodness" of scenarios influence subsequent judgment. Thuring and Jungermann (1986) also discuss scenarios in terms of mental simulation. Scenarios can differ in the degree of emphasis placed on the causal sequence of events versus the end-state or outcome (sometimes called chain versus snapshot scenarios). Other researchers describe scenarios as "flowing narratives" that trace the progression of events (Schnaars & Topol 1987, p.405), stories about the future that are "both causal and concrete" (Kuhn & Sniezek 1996, p.2), "focused descriptions of fundamentally different futures presented in coherent script-like or narrative fashion" (Schoemaker 1993, p.195), or "a narrative description of a consistent set of factors which define in a probabilistic sense alternative sets of future business conditions" (Huss 1988, p.377). While most researchers seem to basically agree about the scenario construct, their operationalizations do differ (as I will later discuss).

I have previously alluded to several unique characteristics of scenarios. First, scenarios include aspects of imagination and explanation. Second, scenarios describe the future using a 'story' schema that links beliefs and inferences together in support of a focal hypothesis. These characteristics of scenarios may contribute to their powerful effects on judgment, and there is some evidence to support the importance of these characteristics of scenarios.

Imagination versus Explanation

As the previous discussion suggests, scenarios seem to include aspects of both imagination and explanation. While there is little prior work on scenarios per se, researchers have investigated these component processes. The main thrust of this prior research is that imagination and one-sided explanation bias judgment.

Imagination

In a typical imagination study, subjects are instructed to read or listen to presented scenarios that describe a chain or sequences of events (e.g., Carroll 1978; Gregory, Cialdini, & Carpenter 1982; Gregory, Burroughs, & Ainslie 1985) or to draw specific scenarios leading to an outcome (Anderson & Godfrey 1987). For example, imagining a presidential election outcome increased its likelihood estimate, regardless of whether subjects generated reasons (Carroll 1978). Imagining affects both outcome probabilities and attitudes indirectly related to the imagined scenario (Gregory et al. 1985). Imagining can also affect behavior. For example, people who imagined having CATV were more likely to predict that they would want CATV and to later subscribe to CATV (Gregory et al. 1982). The target and specificity of the presented script also affect the impact of the imagined scenario on judgment (Anderson & Godfrey 1987). In cases in which subjects generate their own scenarios, the ease or difficulty of imagining mediates effects on judgment. For example, subjects who imagined a disease with easy-to-imagine symptoms provided higher likelihood judgments for contracting the disease than subjects who imagined difficult symptoms (Sherman, Cialdini, Schwartzman, & Reynolds 1985).

Explanation

Most prior research has operationalized one-sided explanation with instructions to read or list reasons (or arguments or causes) favoring one side of an issue. For example, explaining the occurrence of an event increased the judged likelihood of the event (e.g., a patient's suicide), even when subjects knew in advance that the event was hypothetical or when the event was discredited afterwards (Ross, Lepper, Strack, & Steinmetz 1977). Similarly, explanation increased the judged likelihood of a social theory (e.g., the relationship between risk-taking and firefighters), even after the initial evidence was discredited (Anderson, Lepper, & Ross 1980). Explaining a personal event (e.g., success or failure in an anagram task) influenced both expectations and actual behavior (Sherman, Skov, Hervitz, & Stock 1981), a kind of self-fulfilling prophecy (Sherman 1980). All of these studies provide evidence that generating reasons influences judgment and that one-sided explanation can lead to persistent biased judgment (called “belief perseverance”). In these studies, the judgmental bias is typically attributed to the increased availability of information in memory supporting the target event following the explanation task (the so-called availability bias; Tversky & Kahneman 1973).

Because of its nonanalytic nature, imagination (and scenarios) may be more susceptible to salient information, mental imagery and other associations than explanation. Levi and Pryor (1987) compared imagination and explanation by distinguishing between “imagery of the event itself” (outcome imagery) and “reasons or causes for the event”. Subjects were presented with and instructed to imagine scenarios (for the Reagan/Mondale debate winner) containing both outcome imagery and reasons or outcome imagery alone. Imagining scenarios with both outcome imagery and reasons influenced predictions; imagining scenarios with outcome imagery alone had no effect. Unfortunately, the design of the study lacked the control groups necessary to ensure that the scenarios themselves were equally persuasive (without imagining). The authors did find that the effect of imagining the outcome was still significant after controlling for self-generated reasons. However, this result is difficult to interpret. It can suggest either a true effect of outcome imagery in conjunction with reasons or an imperfect measure of self-generated reasons in cognitive responses. The authors also speculate that outcome imagery alone could serve as a vividness manipulation and influence judgment under other circumstances. Some correlational evidence for this speculation can be found in a study of risk assessment. Personal risk ratings for events (e.g., lightning, tornado flood disaster) were higher for groups with prior disaster experience. Imagery ratings mediated both events; memory clarity mediated lightning only (Greening, Dollinger, & Pitz 1996). Although this evidence is fairly weak, other areas of research have found vividness effects in judgment (e.g., Pham 1994; Reyes, Thompson, & Bower 1980; but see Taylor & Thompson 1982).

Based on these findings from the imagination and one-sided explanation literature, it would be unsurprising if scenarios were found to bias judgment. Indeed, this notion is a basic premise of my research propositions. However, the distinction between imagination and explanation may be important when analytic debiasing tools are examined. As I discuss in more detail later, research on one-sided explanation suggests that counterexplanation is effective at debiasing judgment. On the other hand, I am unaware of any findings in the imagination literature that address whether imagination-induced biases can be eliminated by analytic thinking. If imagination-induced biases are difficult to eliminate (which will be investigated in Pilot Study 1), then the effectiveness of analytic reasoning at eliminating scenario-induced biases is an open question.

Scenarios versus Reasons

The storytelling nature of scenario generation may also be an important characteristic that distinguishes scenarios from reasons. Scenarios describe the future using a story schema that links events, beliefs, and inferences together in support of a focal hypothesis. Evidence drawn from disparate sources suggests that events in sequence may be judged in a biased fashion, thereby enhancing a scenario's perceived plausibility and perhaps increasing its resistance to debiasing.

Conjunctive Fallacy

When events are linked, the conjunction is judged more likely than its constituent events, contrary to the laws of probability. One explanation for the conjunctive fallacy is that people tend to assess probability via availability and representativeness (Tversky & Kahneman 1983). Indeed, Sloman (1996) uses the conjunctive fallacy as evidence of the co-existence of analytic and nonanalytic systems of reasoning. Scenarios that provide a causal or temporal sequence to events may be susceptible to this nonanalytic judgmental bias.

Story Construction

In addition, some researchers propose that people spontaneously evaluate evidence through "story construction," attempting to link events together into a causal sequence or scenario that explains the outcome. Evidence for spontaneous story construction and its influence on judgment has been found with novices and experts

(Ricchiute 1998). For example, a shift in evidence order changed jury decisions in the direction of the more easily constructed story (Pennington & Hastie 1986, 1988). Moreover, evidence organized as a story leads to stronger guilt and confidence judgments than evidence organized by issue or reason (motive, character, opportunity) in a jury context (Pennington & Hastie 1992). Interestingly, providing explicit story inference information (for acquittal versus conviction) had a greater impact on global evaluations (based on story construction) than on evaluations of evidence items, thereby supporting the notion that a story is greater than the sum of its arguments. In a similar vein, Teigen (1994) proposes that the plausibility of “fictional events” is a function of completeness, coherence, causal elements, familiarity/acceptability (easily assimilated to existing schemas), explicitness of premises (from which consequences follow naturally), and suggestiveness (gives rise to inferences because it “rings true”).

A scenario or story elaborates and links reasons together in an integrated causal schema that explains an event or outcome. As previous evidence suggests, stories appear to be more powerful than reasons; hence, scenarios may bias judgment. Moreover, the distinction between scenarios and reasons may be important when analytic debiasing tools are examined. Wilson and Brekke (1994) argue that correcting for “mental contamination” depends upon the direction and magnitude of a bias, as well as motivation and ability to correct the bias. Their framework implies that it may be easier to correct for bias due to the generation of reasons than scenarios because the former may be encoded discretely whereas the latter are encoded integratively in an elaborate schema that is difficult to undo. If elaboration of scenarios creates biases that are difficult to eliminate with evenhanded reasons (which will be investigated in Pilot Study 2), then the effectiveness of *other* analytic techniques for eliminating scenario-induced biases merits further investigation.

Debiasing Scenarios with Analytic Thought

Common sense and prior research suggests that analytic thinking improves judgment. Various analytic debiasing tools have been proposed in the literature to improve judgment. One tool consists of evenhanded generation of reasons, the traditional form of analytic thinking. If the source of bias is the increased availability of one-sided evidence, then any technique that increases the availability of evidence supporting the other side should improve judgment. Such techniques might collectively be termed "counterfactual reasoning." Three basic strategies--presenting evenhanded arguments, generating evenhanded arguments, and generating opposing arguments (or counterexplanation)--can be used to make salient both sides of an argument. The basic premise of analytic thinking is that evenhanded consideration of the evidence improves judgment.

Prior research clearly indicates that *counterexplanation* reduces explanation-induced biases. For example, subjects are better calibrated if they list reasons why their answers to almanac questions might be wrong (Koriat et al. 1980). Similarly, generating con reasons improved accuracy in predicting positive personal events (e.g., MBA's starting salary estimates) when the event base rates were low to moderate (Hoch 1985). In these studies, generating supportive reasons had no effect on judgment, suggesting that a confirmation bias may occur spontaneously. These results are also consistent with work by Wilson and LaFleur (1995), who phrased questions to prompt spontaneous generation of one or the other side of the argument, thereby altering the direction of bias. Mahajan (1992) replicated this effect in a marketing context. These studies constitute evidence that counterexplanation, one form of counterfactual reasoning, improves judgment after spontaneous explanation. A key study by Hoch (1984) investigated delay and order effects in deliberate pro and con argument generation for various target events (e.g., OPEC embargo, buying a VCR). Without a delay, net reasons were biased toward initial reasons generated, which Hoch attributes to output interference. With a delay between argument generation tasks, there was no difference in net reasons generated and people achieved evenhandedness. Reasons generation influenced judgment, as expected by an explanation bias. That is, predictions showed a primacy effect with no delay and a recency effect with a delay. Overall, Hoch's work suggests that a delay between pro and con argument generation will improve the effectiveness of counterexplanation in improving judgment.

It is important to note, however, that these results only address the effectiveness of counterexplanation in eliminating *explanation*-induced biases. It does not provide any direct evidence that counterfactual reasoning can eliminate imagination- or scenario-induced biases. Indeed, I have previously argued that the unique characteristics of scenarios may increase their resistance to debiasing with analytic reasoning. Thus, it may be dangerous to extrapolate these findings to scenario contexts. My research is designed to address this limitation.

Of course, common sense suggests that generating evenhanded reasons or considering evenhanded evidence should improve judgment that is biased by one-sided elaboration. However, if counterfactual reasoning is *differentially* effective at eliminating bias depending on its source, then the source of the bias merits further attention. In other words, nonanalytic thinking via scenarios may have consequences for the effectiveness of subsequent analytic thinking. To examine this possibility, two pilot studies were conducted. The first pilot study explores differences in the resistance of imagination- and explanation-induced biases to an evenhanded reason-generation task. The second pilot study extends this investigation of debiasing via evenhanded reasons to scenarios themselves. If imagination and scenario effects prove difficult to eliminate with evenhanded reasoning in the pilot studies, then the effectiveness of other analytic tools merit further investigation.

CHAPTER 3
PILOT STUDY 1: IMAGINATION AND EXPLANATION

As I have argued previously, elements of both imagination and explanation constitute scenarios. In addition, prior research has demonstrated the effectiveness of counterfactual reasoning following explanation only. Thus, it may be unwise to generalize this debiasing effect. If imagination resists debiasing via evenhanded reasoning, then scenario-induced biases merit further investigation. If not, then the distinction between imagination, scenarios, and explanation may be uninteresting from a debiasing standpoint. In other words, this study serves as a pilot to determine whether debiasing scenarios merits further investigation.

This exploratory study investigates the roles of imagination and one-sided explanation in probability judgment and choice among two alternative new product ideas or investment opportunities. Some subjects were asked to engage in explanation and others were asked to engage in imagination. A second factor in this study can be viewed as a manipulation of the strength of the subsequent analytic mindset induced at choice. In the explicit reasoning condition, subjects were first asked to list the relative merits of the two alternatives before making their choice. In the other condition, subjects were simply presented with the alternative new product ideas and asked to make a choice. Explicitly listing reasons before choice may increase the amount of analytic thinking as well as the salience and importance of these reasons in the decision process. Doing so could reduce the impact of imagination on judgment and choice, although there does not appear to be any reason why it should differentially reduce bias due to one-sided explanation versus imagination.

Most prior studies of imagination and explanation have not used a time delay (but see Gregory et al. 1982, 1985) or a choice context, relying instead on probability and attitudinal judgments. A choice between two alternatives may place managers in an analytic mindset, in which generating reasons for the alternatives is the most likely response. In addition, a delay leads to forgetting and reduces the accessibility of one-sided evidence, thereby reducing output interference effects (in which initially accessible information inhibits the retrieval of other information) and potentially improving the evenhandedness of reasoning. These conditions, then, strengthen analytic reasoning prior to judgment and provide a relatively conservative test of differences in the persistence of imagination and explanation effects.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of four cells in a 2 (Type of elaboration: imagine versus explain) x 2 (Explicit reasoning versus not) between-subjects design. A total of 52 subjects participated, with the number in each cell ranging from 11 to 15.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. The procedure took place over two sessions, with a one-week delay between sessions. In the first session, the survey provided a short description of a new product (the electric car), and asked subjects to elaborate in one of two ways. In the imagine condition, subjects were instructed as follows:

> Please take a few moments to imagine what it would be like if this product existed and was widely adopted in the marketplace. For example, imagine what it would be like to use this product and/or what our world would be like if most cars were electric. Form a mental picture of yourself in this future world where electric cars are in widespread use. Then, write down your vision in the space below.

In the explain condition, subjects were instructed as follows:

> Please take a few moments to think of reasons why this product might succeed in the marketplace. For example, what are some of the reasons why consumers would buy this product? Write down these reasons in the space below.

Subjects were given a fixed amount of time to elaborate before working through the rest of the materials at their own pace. Subjects then rated their attitude toward electric cars, likelihood of purchase if available, and prior knowledge. After a delay of one week, subjects then returned to the laboratory and were presented with a second survey. This survey asked subjects to play the role of a senior manager at Royal Dutch/Shell Corporation and to consider two possible investment opportunities: investment in a battery manufacturing firm (for electric cars) or investment in a photovoltaic cell manufacturing firm (for solar-based home electricity). After reading a description of the investment opportunities, half of the subjects (in the explicit reasoning condition) were asked to write down their thoughts regarding the relative merits of the two alternatives while the remaining subjects proceeded directly to additional dependent measures. These measures took the form of several questions that asked subjects to recommend an investment opportunity, rate their confidence regarding that recommendation (between 50% and 100%), and rate the percentage chance of success of each opportunity (between 0 and 100%) over the next 5 years, 10 years, 20 years, and beyond 20 years. A final thought-listing question asked subjects for the rationale behind their investment choice.

Results and Discussion

CATMOD and LOGIT analyses revealed no statistically significant difference in the effect of the initial type of elaboration (imagine or explain) on choice, although the difference in choice is in the direction predicted (see Table 2). ANOVA revealed a main effect of choice on confidence ($F_{1,44}=5.75$, $p<.021$). Subjects who chose the imagined or explained alternative were more confident than subjects who chose the other alternative. However, the expected interaction between the alternative chosen and the type of elaboration was not significant ($F<1$). Imagining did not lead to greater confidence than explaining when the imagined/explained alternative was chosen. No significant effects for the explicit reasoning factor were found in these analyses.

Subjects were also asked to rate the percentage chance of success of each opportunity (between 0 and 100%) over the next 5 years, 10 years, 20 years, and beyond 20 years. ANOVAs with success estimation time period as a within-subject factor and type of elaboration and explicit reasoning as between-subjects factors were conducted for each investment opportunity. First, consider the predictions for the electric car. As one would expect, estimates of success increase with time period ($F_{1,43}$=269.17, p=.0001). This linear trend is qualified by an interaction with type of elaboration ($F_{1,43}$=7.35, p=.0096); similarly, the cubic time trend is significant ($F_{1,43}$=6.11, p=.0175) and qualified by an interaction with type of elaboration ($F_{1,43}$=7.69, p=.008). As a graph of the means for each time period indicates, the slope for predictions is greater in the imagine condition than in the explain condition (see Table 3 and Figure 1). A similar analysis of the predictions for the alternative investment opportunity, solar home electricity, indicated that all effects were non-significant, as expected.[4] These results suggest that imagining an alternative leads to more optimistic predictions regarding success of that alternative over time. It is interesting to note that estimates of future success appear to be higher for 5 and 10 years after explaining, and higher for 20 years and beyond after imagining. A univariate follow-up analysis of predictions for each time period revealed a significant effect of type of elaboration for beyond 20 years only ($F_{1,43}$=4.05, p=.051).[5] This suggests that explanation may have a greater effect on short-term judgments and imagination may have a greater effect on long-term judgments, perhaps because subjects may be more inclined to rely on nonanalytic judgments than rational analysis given the uncertainty inherent in the long-term judgment task.

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[4] A MANOVA of both sets of predictions, with investment opportunity and estimation time period as within-subject factors and type of elaboration and explicit reasoning as between-subjects factors, revealed the following significant effects: investment opportunity ($F_{1,43}$=3.819, p=.057), time ($F_{1,43}$=450.07, p=.0001), time X type of elaboration ($F_{1,43}$=5.13, p=.029), time^3 X type of elaboration ($F_{1,43}$=3.41, p=.072), investment opportunity X type of elaboration X time ($F_{1,43}$=3.51, p=.068) and investment opportunity X type of elaboration X time^3 ($F_{1,43}$=7.18, p=.010). These latter two interactions with investment opportunity indicate that there are significant differences in the effect of type of elaboration on predictions for the imagined/explained investment opportunity and the alternative opportunity, as one would expect. For expository clarity, the results of the two separate ANOVAs for each investment opportunity are reported in the main text.

In none of these analyses was there a significant effect for the explicit reasoning factor. These findings are notable because they indicate that the effects of imagining endure despite attempts to induce an analytic mindset with or without explicit reasoning at the time of choice. On the other hand, null effects must be interpreted with caution.

Overall, these results provide partial support for the hypotheses. Although the imagined alternative was not chosen significantly more often nor with more confidence than the explained alternative, there was a significant effect on subjective probability judgments concerning future success of the alternative. The most important finding is that imagining the alternative led to more optimistic predictions of future success than explaining the alternative. Even after a one-week delay and an explicit evenhanded reasoning task, subjects were influenced by a prior imagination/ explanation task that lasted only a few minutes. Inasmuch as imagination is a characteristic element of scenarios, then scenarios may also have enduring effects that resist debiasing with evenhanded reasoning.

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[5] Explicit reasoning is marginally significant for predictions for the next 5 years ($F_{1,43}$=3.28, p=.077): subjects who listed reasons for choice provided lower estimates for the first time period only.

Table 2.
Pilot study 1: choice and confidence after imagining/explaining success of a target alternative.

Type of Elaboration	Reasoning at Choice	CHOICE (of target alternative)	CONFIDENCE (in choice of target alternative	CONFIDENCE (in choice of non-target alternative
Imagine	Not explicit	6 / 11	84.09	71.25
		73.3%	(8.01)	(11.09)
Imagine	Explicit	9 / 15	83.33	78.33
		60.0%	(13.46)	(12.11)
Explain	Not explicit	6 / 11	77.50	69.00
		54.5%	(12.55)	(14.75)
Explain	Explicit	6 / 11	80.83	75.00
		54.5%	(8.61)	(12.75)

Table 3.
Pilot study 1: probability of success over time after imagining/explaining success of a target alternative.

Time Period:	over next 5 years	over next 10 years	over next 20 years	beyond 20 years
Imagine	13.70	33.89	67.93	88.00
	(13.34)	(19.58)	(23.86)	(16.33)
Explain	20.25	40.65	58.60	75.70
	(20.60)	(22.08)	(21.95)	(25.15)

Note: For purposes of expository clarity, the results are not displayed as a function of explicit reasoning.

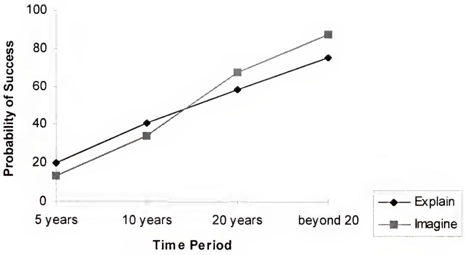

Figure 1.
Pilot study 1: probability of success over time after imagining/explaining success of a target alternative.

CHAPTER 4

PILOT STUDY 2: SCENARIOS AND ELABORATION

In the second pilot study, my investigation shifts from imagination to formal *scenarios*, the focus of the first part of this dissertation. The objective of this pilot study is twofold. First, it is designed to demonstrate the resistance of *scenarios* to debiasing with evenhanded reasoning. This objective follows logically from the previous study, which demonstrated the resistance of *imagination* (a unique characteristic of scenarios) to debiasing with evenhanded reasons. If evenhanded reasons fail to eliminate scenario effects, then other analytic tools merit investigation. Second, this study is designed to demonstrate that *elaboration* of a scenario is a source of persistence. Prior research suggests that stories are stronger than reasons, and I have argued previously that scenario generation leads to the construction of an elaborate story schema to support the focal hypothesis, i.e., success or failure. Thus, further elaboration of this scenario should bolster support for the hypothesis and polarize judgment. Insofar as people tend to think about and elaborate on a scenario, then this could account for the persistence of scenario-induced biases over time.

Support for the biasing effect of additional thought is drawn from the polarization literature, which suggests that directed (or one-sided) thought leads to polarized judgment. In this stream of research, Tesser and Leone (1977) initially demonstrated that people make more polarized judgments for attitude objects after schema-driven thought. This finding contrasted with later work by Linville and others, who demonstrated that attitude polarization depends on whether the schema is simple or complex (Linville & Jones 1980; Linville 1982). To reconcile these findings, Millar and Tesser (1986) proposed that commitment and cognitive structure account for the contradictory findings of Tesser and Linville. When people are committed to initial evaluations, thought leads to polarization in the presence of more complex schemas. When uncommitted, thought leads to polarization when driven by a simple versus complex schema. In these studies, commitment and the cognitive structure can be viewed as influencing the direction of thought. Similarly, adopting a focal hypothesis and elaborating on this hypothesis (i.e., constructing a story or scenario) should also induce directional thought and lead to polarized judgment.

In this pilot study, subjects were instructed to imagine either new product success or failure. Some subjects were also given the opportunity to carry out further elaboration of the initial scenario. As in the previous study, I predict that the scenario-induced bias will endure despite subsequent instructions to generate evenhanded pro and con reasons. Moreover, additional thought or elaboration of a scenario will enhance its effect on judgment.

By contrasting success and failure scenarios, the effects of imagination can be generalized to both success and failure scenarios. In this study, subjects were also given a lengthy and evenhanded new product description (consistent with prior research on story construction), in contrast to the short description provided in the previous study.[6] Because the results in the previous study were somewhat sensitive to the type of dependent variable, I included additional dependent measures related to new product success. Whereas the first pilot study manipulated the strength of analytic thinking in an attempt to demonstrate differences in debiasing, this study manipulated the strength of the imagined scenario in an attempt to demonstrate differences in induced bias.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of four cells in a 2 (Type of scenario: success versus failure) x 2 (Amount of elaboration: low versus high) between-subjects design. A total of 48 subjects participated, with the number in each cell ranging from 10 to 13.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. Subjects worked through the materials at their own pace. Subjects played the role of a new product manager and read a three-page description of the current status of a new product idea (electronic books). The description was deliberately designed to be neutral and to provide subjects with mixed information regarding the industry players and customers for the new product. Subjects indicated their initial opinion of the new product idea and rated the product information. They then were instructed to take at least 5-10 minutes to write either a success or failure scenario as follows:

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[6] To increase the generalizability of results across studies and to simplify this study's design, this study did not incorporate a delay or choice and used a different target new product in the stimulus materials.

> Failure Scenario: You have just read a report that gives you some background information about the current status of Electronic Books. Now we would like you to take some time to think about a worst-case scenario in which E-Books will *not* succeed in the future. (A scenario is a story that describes how events unfold over time and includes vivid details that create a mental picture or vision of the future.) Write a scenario that describes a story of failure for E-Books.
>
> Success Scenario: You have just read a report that gives you some background information about the current status of Electronic Books. Now we would like you to take some time to think about a best-case scenario in which E-Books will succeed in the future. (A scenario is a story that describes how events unfold over time and includes vivid details that create a mental picture or vision of the future.) Write a scenario that describes a success story for E-Books.

After writing this scenario, subjects indicated their opinion again and rated various aspects of their written scenario. These materials were then removed and subjects completed a short filler task. Subjects in the high elaboration conditions were then asked to elaborate on their scenario as follows:

> In scenario planning, people usually find that it is worthwhile to spend additional time working on their scenario. Please take a few more minutes to think about the scenario that you wrote about Electronic Books. Try to recall it in as much detail as you can. Feel free to fill in details and elaborate on events in your scenario. Work on developing your scenario more fully. Write down this scenario in the space below. (If you need more space, please use the other side of the page.)

Subjects in the low elaboration condition proceeded directly to a second filler task.

After a second short filler task, all subjects generated evenhanded reasons for the success and failure of E-Books. Subjects then indicated their opinion again and answered various other questions regarding the future of E-Books.

Results and Discussion

This study predicted a two-way interaction between the type of scenario and the amount of elaboration. Additional elaboration of the scenario was expected to exacerbate the difference between judgments following a success versus failure scenario. The overall pattern of results across the dependent measures is consistent with this hypothesis.

Consider first several direct measures of attitudes toward E-Books. Subjects were asked to rate their agreement with several statements concerning the success of E-Books: "E-Books are destined to succeed in the marketplace," "E-Book sales will take off and grow rapidly," "People will be reluctant to try this new technology," and "After some initial interest, E-Book sales are likely to stall in the marketplace." (The final two statements were reverse-coded.) A MANOVA revealed a significant interaction of the type of scenario and the amount of elaboration ($F_{1,44}$=5.36, p=.025), as well as a main effect of the type of scenario ($F_{1,44}$=4.93, p=.032). As the means in Table 4 indicate, subjects were less optimistic about the E-Book's success if they generated *and elaborated* on a failure versus success scenario. Follow-up analyses of the high elaboration groups reveals a significant effect of type of scenario ($F_{1,44}$=10.82, p=.002); otherwise, there was no scenario effect (F<1). Thus, elaboration polarized judgments; as predicted, generating evenhanded reasons was insufficient to eliminate the bias if the scenario was given additional elaboration.

This pattern of results is also found on several other less direct judgment measures (see also Table 4). For example, subjects were asked to predict attitudes toward Internet shopping for a number of different groups (e.g., adults, teens, seniors, schools, publishing companies, retailers, etc.). A MANOVA of these predictions revealed a significant two-way interaction of type of scenario and amount of elaboration ($F_{1,44}$=3.38, p=.073), as well as a main effect of type of scenario ($F_{1,44}$=7.58, p=.009). In planned contrasts, the effect of type of scenario was significant under high elaboration ($F_{1,44}$=11.09, p=.002); otherwise, type of scenario had no effect (F<1). In other words, predictions were more (less) favorable toward Internet shopping when subjects generated *and elaborated* on a success (failure) scenario.[7] A similar pattern of means is evident in predictions for the attractiveness in electronic form of various reading materials (e.g., best-sellers, classic literature, children's books, school text books, business books, popular magazines, professional magazines and newsletters, etc.) but does not reach significance. Personal attitudes were also affected by a two-way interaction of the type of scenario and amount of elaboration ($F_{1,44}$=5.14, p=.028). Elaborating about a success (failure) scenario led to more (less) positive personal opinions about E-Books ($F_{1,44}$=6.46, p=.015); this difference was not significant in the low elaboration groups (F<1). Thus, results so far provide strong support for the hypothesis.

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[7] For brevity's sake, these results are not reported separately in tables in this chapter. Results are available from the author upon request.

Some additional dependent measures were taken repeatedly and their analysis deserves special attention. First, subjects' initial attitude toward E-Books (after reading the background information) did not differ across groups (all effects were non-significant). Second, the initial scenario generation task had an immediate effect: attitudes were more (less) favorable ($F_{1,41}$=22.31, p=.0001) and predictions were more (less) optimistic ($F_{1,41}$=10.85, p=.002) after a success (failure) scenario. Third, attitudes were not consistently different after the reason generation task. Attitudes were slightly more favorable for subjects who generated a success, versus failure, scenario ($F_{1,44}$=3.43, p=.071) but no more optimistic; contrary to the hypothesis, additional elaboration had no effect (although the means in Table 5 are in the expected direction). Finally, this same pattern is evident when these attitudes were measured again later in the survey (all F<1). However, at this point, an additional attitude measure that asked subjects to assess the rate of growth of E-Books did reveal the hypothesized two-way interaction ($F_{1,43}$=5.09, p=.029; see Figure 2). Subjects assessed a more rapid growth rate after generating *and elaborating* about a success, versus failure, scenario ($F_{1,44}$=7.21, p=.010). The growth rate did not differ for subjects who did not carry out additional elaboration (F<1). This significant interaction effect for growth rate reflects the same pattern of means found on the non-repeated measures discussed previously.

Subjects were also asked to predict the percentage of Americans that will adopt E-Books over time (see Table 6). A MANOVA of these estimates revealed a significant three-way interaction of type of scenario, amount of elaboration and estimation period (cubic polynomial; $F_{1,44}=6.51$, $p=.014$). Follow-up analyses revealed marginal interactions of type of scenario and estimation period under both high and low elaboration (cubic polynomial; high: $F_{1,44}=2.95$, $p=.093$; low: $F_{1,44}=3.56$, $p=.066$). Unexpectedly, the pattern of means is in the opposite direction: low elaboration produced larger scenario differences than high elaboration. Subjects were also asked to estimate market share for electronic book materials over time (see also Table 6). A MANOVA reveals an interaction of the amount of elaboration with estimation period (cubic polynomial; $F_{1,44}=4.76$, $p=.035$), also qualified by an interaction with the type of scenario ($F_{1,44}=4.67$, $p=.036$). In planned contrasts in both elaboration conditions, the interaction of type of scenario and estimation period failed to reach significance (cubic polynomial; high: $F_{1,44}=2.56$, $p=.117$; low: $F_{1,44}=2.14$, $p=.151$), probably due to low power. As the pattern of means for the significant two-way interaction suggests, estimates were more (less) optimistic following generation *and elaboration* of a success (failure) scenario. This latter result is supportive.

Overall, these results indicate that elaboration about scenarios leads to polarized judgments. In addition, generating evenhanded reasons was insufficient to eliminate a scenario-induced bias after additional elaboration. However, generating pro and con reasons was effective at debiasing judgment when scenario elaboration was limited. From a practical standpoint, the latter result can be taken as evidence that the reason generation task was an effective manipulation (countering the usual criticisms of null effects and demand). From a theoretical standpoint, this study provides a boundary condition on scenario effects by suggesting that the power of scenarios lies in their elaboration. Elaborate scenarios resist debiasing via evenhanded reasoning; the question then turns to whether other analytic tools are effective.

Table 4.
Pilot study 2: judgments after generating and elaborating a scenario for failure/success of electronic books.

Type of Scenario	Amount of Elaboration	N	Agreement with Success Statements (Averaged)	Predictions of Others' Attitudes (Averaged)	Attractiveness of Reading Materials (Averaged)	Personal Attitudes (Averaged)
Failure	High	13	2.40 (0.71)	3.72 (0.67)	4.45 (1.09)	4.19 (1.74)
Failure	Low	13	2.61 (0.35)	3.98 (0.63)	4.60 (0.96)	4.90 (1.28)
Success	High	12	3.15 (0.42)	4.47 (0.55)	4.97 (0.86)	5.56 (1.02)
Success	Low	10	2.60 (0.71)	4.13 (0.27)	4.53 (0.60)	4.50 (1.18)

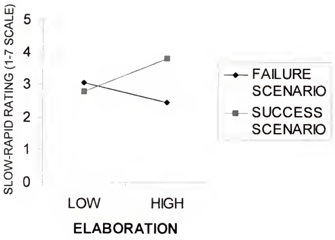

Figure 2.
Pilot study 2: growth rate of electronic books after scenario elaboration and evenhanded reasons generation.

Table 5.
Pilot study 2: attitude ratings measured after scenario and pro/con generation and then repeated after additional questioning.

Type of Scenario	Amount of Elaboration	Attitude After Pro/Con Generation			
		N	Favorability	Success	Growth Rate
Failure	High	13	3.38 (1.61)	3.62 (1.80)	2.77 (1.59)
Failure	Low	13	4.00 (1.00)	4.15 (1.28)	3.23 (1.54)
Success	High	12	4.50 (1.51)	4.50 (1.17)	3.08 (1.08)
Success	Low	10	4.30 (0.95)	4.20 (1.03)	2.70 (0.82)
		Attitude Repeated After Additional Questioning			
		N	Favorability	Success	Growth Rate
Failure	High	13	3.54 (1.71)	3.92 (2.06)	2.46 (1.56)
Failure	Low	13	4.15 (1.28)	4.23 (1.54)	3.08 (1.12)
Success	High	11	4.18 (1.54)	4.73 (1.35)	3.82 (1.08)
Success	Low	10	4.30 (1.16)	4.30 (1.16)	2.80 (1.03)

Note: Initial attitudes and post-scenario attitudes are not reported due to space limitations.

Table 6.
Pilot study 2: adoption and market share estimates over time for electronic books.

Type of Scenario	Amount of Elaboration	N	% of Americans Adopting Ebooks over Time			
			2005	2010	2020	Beyond 2020
Failure	High	13	13.69	25.77	44.38	57.23
			(20.37)	(23.26)	(27.05)	(28.21)
Failure	Low	13	15.85	28.85	43.08	53.85
			(18.37)	(20.79)	(28.74)	(38.31)
Success	High	12	17.75	30.92	45.00	62.08
			(20.15)	(30.21)	(31.41)	(30.49)
Success	Low	10	20.69	33.62	57.37	70.62
			(18.09)	(20.40)	(27.29)	(32.41)
			% Market Share for Electronic Books over Time			
Failure	High	13	20.38	25.54	38.77	47.38
			(28.32)	(18.16)	(27.11)	(32.52)
Failure	Low	13	23.55	31.77	46.92	56.15
			(26.35)	(25.07)	(33.86)	(39.06)
Success	High	12	19.25	35.58	49.84	66.76
			(17.83)	(29.61)	(31.57)	(31.57)
Success	Low	10	18.20	29.40	55.30	66.30
			(17.29)	(19.13)	(28.28)	(30.41)

CHAPTER 5
STUDY 1: SCENARIO ANALYSIS

The pilot studies suggest that imagination and elaboration contribute to scenario effects on judgment. These studies also provide evidence that one debiasing tool, evenhanded reason generation, failed to eliminate the effects of scenarios on judgment. In light of these persistent effects of scenarios, Study 1 strengthens debiasing efforts by using two additional analytic tools, counterfactual reasoning and presented evidence, in an effort to eliminate scenario effects on judgment. Counterfactual reasoning heightens the availability of opposing information and therefore can be viewed as a stronger debiasing tool compared to evenhanded reasons (as in the pilot studies). From an availability perspective, presented evidence is also a stronger manipulation of analytic thinking compared to generated evidence because it eliminates memory-mediated effects that reduce the accessibility of opposing arguments after a one-sided elaboration task. In this study, both of these analytic tools are used in an attempt to improve judgment. Thus, this study provides the strongest test yet of scenario-induced biases and the effectiveness of analytic tools in debiasing judgment.

This study also offers direct evidence of "bias." Heretofore, I have demonstrated scenario "effects" on judgment, which allows me to conclude that judgments are flawed (inasmuch as differential predictions cannot all be accurate). In the absence of actual outcome data (which is not available for these new products), the accuracy of predictions cannot be ascertained nor can "bias" (which exists when predictions systematically differ from actual outcomes). However, judgment can be called biased if it is path dependent. That is, processing order in judgment is causally unrelated to actual outcomes, thereby inducing systematic effects on judgment that constitute bias.

In addition, this study is designed to investigate scenario analysis, a management tool for improving forecasting. In scenario analysis, managers first generate several alternative future scenarios and then forecast the future. Proponents of scenario analysis argue that scenarios help managers deal with an uncertain future by simplifying it into a limited number of alternatives (Wack 1985). The main benefits of scenario analysis from a debiasing perspective are said to include a reduction in both overconfidence and myopic framing (Schoemaker 1992, 1995). From a psychological perspective, the effectiveness of scenario analysis may be rooted in two processes:

1. counterfactual reasoning, mediated by availability of arguments (e.g., Hoch 1984)
2. multiple explanation, mediated by the ease of simulation of alternative outcomes (e.g., Hirt & Markman 1995; Koehler 1991, 1994).

I have already discussed prior psychological research related to counterfactual reasoning. The effectiveness of counterfactual reasoning is mediated by the availability of supporting and opposing arguments that influence judgment. In contrast, research on multiple alternatives suggests that considering *alternative* hypotheses (whether opposing or not) is sufficient to improve judgment. In the discussion that follows, I will review prior research in this area.

Multiple Explanation

Hypothesis Generation

People generally experience difficulty in generating alternatives. They stop too soon when generating alternatives (Shaklee & Fischhoff 1982), generate new hypotheses only when existing hypotheses seem implausible (Gettys & Fisher 1979) and underestimate the other ungenerated hypotheses (Gettys, Mehle, & Fisher 1986). On the bright side, when people do generate alternative hypotheses, they become better calibrated in their judgments. Asking people to generate hypotheses in the other category reduces (but does not eliminate) overconfidence in a focal hypothesis (Mehle 1981). In general, the fault tree literature demonstrates that people do not spontaneously generate other hypotheses or alternatives but when prompted to do so are less overconfident. For example, likelihood estimates for a focal cause (e.g., Bill's death due to smoke inhalation) are reduced by the number of other causes generated and are sensitive to the likelihood of alternative causes (Dougherty, Gettys, & Thomas 1997). However, generating alternatives appears to be a difficult task. Both experts and novices experience difficulty generating a complete set of hypotheses and therefore are overconfident in their judgments (Fischhoff, Slovic, & Lichtenstein 1978; Mehle 1982). Moreover, incentives do not improve performance (Gettys, Pliske, Manning, & Casey 1987). People's inability to generate an appropriate set of alternatives has important implications in a judgment or prediction context. For example, Dube-Rioux and Russo (1988) found that managers generated more short-term than long-term causes of restaurant failure, which led to a greater under-estimation bias for long-term causes of failure. In addition, a causal focus developed early in hypothesis generation (for solutions to parking problems) influences subsequent hypotheses generated (Adelman, Gualtieri, & Stanford 1995). This result is consistent with other research suggesting that people have difficulty generating alternatives because alternatives generated earlier inhibit subsequent idea generation by restricting thinking. For example, people tend to imitate an exemplar solution when designing bicycle racks (Jansson & Smith 1991) or drawing alien creatures (Ward 1994; Marsh, Landau, & Hicks 1996), which suggests the importance of the starting point or initial condition in any generation task.

Hypothesis Evaluation

People also experience difficulty in evaluating multiple alternatives. They exhibit confirmation bias in the search for evidence (Klayman & Ha 1989) and fail to consider how evidence supports other alternatives during testing (Tversky & Koehler 1994; Griffin & Tversky 1992). Moreover, both novices and experts are imperfect hypothesis testers. For example, Asare and Wright (1995) showed that, although expert auditors did not persevere in beliefs and were not influenced by nondiagnostic evidence, they did encode evidence and evaluate hypotheses independently (one hypothesis syndrome) and sometimes resuscitated eliminated hypotheses. Novices made all these normative errors and additional substantive mistakes as well. Hence, people are non-Bayesian when it comes to hypothesis evaluation (see Fischhoff & Beyth-Marom 1983 for a review), which has important implications for debiasing. For example, evidence increasing the suspiciousness of one suspect in a mystery story does not decrease the suspiciousness of alternative suspects (Van Wallendael & Hastie 1990). Similarly, subjects under- (over-) estimate a focal candidate's job chances when evidence in the candidate's favor is weak (strong), because they fail to consider that the evidence is weak (strong) for the other candidates as well (Sanbonmatsu, Posavac, & Stasney 1997). In other words, people seem to treat multiple alternatives in hypothesis testing as independent, which suggests that instructing people to evaluate how evidence supports the alternatives may be a debiasing strategy. In research by Gibson, Sanbonmatsu, and Posavac (1997), estimating a focal team's probability of winning led to more gambling, compared to a control group that provided estimates for all of the teams. Here, estimating the alternatives debiased judgment, perhaps by forcing people to consider evidence for each of the alternatives rather than selectively testing the focal alternative. Similarly, choosing an answer among alternatives may reduce overconfidence compared to merely evaluating pre-selected answers to general knowledge questions and predictions of basketball games (Ronis & Yates 1987; Sniezek, Paese, & Switzer 1990). These results suggest that any task that leads people to consider evidence for all of the alternatives, rather than just testing the focal alternative, will improve judgment.

Multiple Alternatives

Hirt and Markman (1995) provide a powerful demonstration of the effectiveness of multiple alternatives in debiasing judgment. For example, subjects who explained a ‘close win’ by team A rated team A’s chances of winning as higher. Predictions were subsequently debiased if subjects then explained a ‘close win’ by the opposing team (a traditional counterfactual reasoning task) or a ‘convincing win’ by team A (an alternative explanation task). Explaining an implausible alternative outcome (e.g., a low-ranked team wins its league) failed to debias judgment, which suggests a mediating role of ease-of-simulation (Hirt & Markman 1995). Judgment also improved after a multiple explanation task involving positive, negative, and zero relationships between risk-taking and firefighting (Hirt & Markman 1995; see also Anderson & Sechler 1986 for a counterexplanation version of this study). Hirt and Markman argue that generating alternative explanations undermines confidence in the focal hypothesis, thereby improving judgment.

Koehler (1994) proposes that the consideration of alternatives improves judgment by breaking the conditional frame of reference induced by the focal hypothesis. Subjects who generated their own alternatives (e.g., predicting Academy Award winners, answering general knowledge questions) were better calibrated than subjects who evaluated presented alternatives. Koehler argues that having to generate the alternative increases the *salience* of multiple contenders, thereby improving judgment. Indeed, when the salience of alternatives was held constant by presenting all of the alternatives, choosing among alternatives increased overconfidence compared to evaluating pre-selected answers (Koehler 1994). Koehler argues that the act of choosing increased supportive arguments in favor of one's choice, an effect opposite that found in Ronis and Yates (1987) and Sniezek et al. (1990). It is difficult to reconcile these results with Hirt and Markman's finding, unless something beyond the mere salience of alternatives is necessary to break the inertia of a conditional frame of reference.

Interestingly, some evidence suggests that people may not always be appropriately sensitive to multiple alternatives, making it more difficult to break this inertia. For example, people identified a person checking window locks in a story as a window installer and ignored cues concerning a burglar (Ross & Murphy 1996). Thus, the mere salience of alternatives (generated or presented) may be insufficient to debias judgment, unless sufficient attention is drawn to the alternative perhaps (for example, via an explanation task, as in Hirt & Markman 1995). Indeed, Hirt and Markman's finding is especially interesting in light of the fact that it was based on a similar study conducted by Anderson and Sechler (1986). In this study, predictions for various social theories (e.g., positive versus negative relationship between fire-fighting and risk-taking) changed in the direction indicated by an explanation task. While a counterexplanation task was completely effective in debiasing judgment, exposing subjects to contradictory data (e.g., zero relationship) did not completely debias judgment. In this case, subjects moderated their beliefs somewhat but did not abandon them, perhaps because people are reluctant to replace a strong belief in a relationship with a null belief or because the null data was especially susceptible to biased interpretation. In addition, there may be differences in the impact of arguments that are presented versus generated.

Nonetheless, Hirt and Markman's finding does provide important evidence that multiple alternatives are sufficient to debias judgment. Although researchers have proposed that multiple alternatives debias judgment by improving the evaluation of alternatives or breaking the conditional frame of reference, it can be difficult to distinguish the proposed mechanisms in practice because the former subsumes the latter. Regardless of the proposed mechanism, research on multiple explanation has typically focused on analytic reasoning tasks and may not be safely generalized to nonanalytic thinking.

Scenario Analysis

The premise of scenario analysis is that prompting people to consider alternative future scenarios will improve judgment. Several scenario studies support this contention and include some evidence that multiple scenarios reduce overconfidence. For example, instructing subjects (somewhat heavy-handedly) to stretch their estimate limits by considering best-case and worst-case outcomes increased confidence intervals for duration of a software project (Connolly & Dean 1997). Similarly, subjects prompted to make multiple construals widened their confidence intervals for self and other predictions (e.g., money spent on dinner, time spent answering phone survey) compared to other subjects (Griffin, Dunning, & Ross 1990). Evidence from the control groups suggests that people typically generate a single construal of a situation and make predictions as if this construal were certain/correct. Schoemaker (1993) also found that both self-generated and presented scenarios led to wider confidence ranges. However, scenarios did not always reduce overconfidence, perhaps because they were sometimes perceived as extreme and therefore implausible. Interestingly, the beneficial effects of scenario analysis were obtained even though subjects still exhibited desirability biases, favored positive trends and outcomes more highly, and were susceptible to the conjunctive fallacy. These judgmental errors seem to suggest the fallibility of scenario analysis as an analytic tool to improve judgment.

Several other studies are only weakly supportive, if at all, for scenario analysis. For example, subjects who made a series of predictions based on graphical and scenario information exhibited increased confidence with multiple scenarios, but accuracy did not improve correspondingly (Schnaars & Topol 1987). Similarly, Kuhn and Sniezek (1996) found that confidence in prediction increased with any type of presented scenarios (unidirectional, conflicting or hybrid scenarios, compared to no scenario) whereas point estimate forecasts and confidence intervals were unaffected. In another study that investigated personal predictions (Byram 1997), subjects who estimated the time required to assemble a computer stand provided “best guess” predictions that were closer to optimistic than pessimistic scenario predictions. There was no evidence of order effects when generating these multiple predictions, and generating multiple estimates (optimistic, pessimistic and best-guess versus best-guess only) did not improve judgment. This optimism bias is reminiscent of the planning fallacy, in which people tend to focus on plan-based scenarios rather than considering the range of past outcomes (e.g., Buehler, Griffin, & Ross 1994).

Taken together, these findings raise questions regarding the efficacy of scenario analysis. In these various studies, the operationalization of scenario analysis ranged from fairly elaborate scenarios generated by subjects (e.g., Schoemaker 1995; Griffin et al. 1990), to simple reasons presented to subjects (Schnaars & Topol 1987; Kuhn & Sniezek 1996), to little more than the brief consideration of the range of possible outcomes (Byram 1997). Based on this distinction, only *elaborate* multiple scenarios improved judgment; mere salience or simple consideration of alternatives was insufficient.[8] Because proponents of scenario analysis typically describe scenario development as a relatively effortful process, results of studies that have used more elaborate scenarios seem more relevant tests of the technique's effectiveness. Most of the studies using elaborate scenarios have been supportive (Connolly & Dean 1997; Schoemaker 1993; Griffin et al. 1990; Hirt & Markman 1995), although some are not (Schnaars & Topol 1987; Kuhn & Sniezek 1996). The latter, however, differed in two aspects: the scenario information was *presented* and resembled *reasons* rather than true scenarios. Of course, other differences among the studies, such as the type of judgment (e.g., personal versus impersonal predictions), increase the difficulty of interpreting and reconciling these contradictory findings.

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[8] As previously discussed, Koehler (1994) argues that mere salience is sufficient to debias judgment. Perhaps the difficulty of generating one's own hypothesis (when no alternatives are made salient), rather than the salience of alternative hypotheses arising out of generation, accounted for reduced confidence in judgment (consistent with his experiments 1, 4, 5 and 6). When alternatives were held salient, choosing (versus evaluating) increased confidence, perhaps via the increased availability of supportive arguments (experiments 2 and 3). The latter is also consistent with Ronis and Yates (1987) and Sniezek et al. (1990).

Perhaps the litmus test for the effectiveness of scenario analysis is whether it is possible to generate and evaluate multiple scenarios in an unbiased manner. Biases in the generation and evaluation process would undermine the effectiveness of scenario analysis as an aid to judgmental forecasting. Prior research has found order effects in generating reasons (e.g., Hoch 1984). Prior research on story construction, as well as results from the pilot studies, suggests that the effect of scenarios may be stronger than the effect of reasons. If so, order effects may be more pronounced for scenarios than reasons. Once an elaborate and integrated schema has been developed in support of an initial scenario, it may be difficult to 'undo' this scenario completely. Based on this line of reasoning and my prior evidence, I predict that scenario analysis will be susceptible to order effects. That is, predictions will be biased in the direction of the initial scenario imagined (primacy order effect). Generating counter-scenarios will not debias judgment.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of four cells in a 2 (Type of elaboration: generate scenarios versus reasons) x 2 (Order: success then failure versus failure then success) x 2 (Repeat predictions: after scenarios and after presented evidence) mixed design. A total of 92 subjects participated, with the number in each cell ranging from 21 to 25.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. The survey asked subjects to play the role of a business executive and think about the future of Internet shopping. Subjects were given some background information on the current status of Internet shopping and asked to give an initial professional opinion. Some subjects then generated a best-case scenario for the success of Internet shopping; other subjects generated reasons for the success of Internet shopping. Subjects then generated a scenario or reasons for failure. Order was manipulated between-subjects, so other subjects generated reasons/scenarios for failure followed by success. (The reasons conditions serve as control groups in this design.)

Each writing task was timed and lasted 5 minutes. In the success scenario writing task, the title of the writing task was "Best-Case Scenario: Internet Shopping is a Success." Subjects were instructed as follows:

> Please think about a best-case scenario in which Internet shopping will succeed. (Recall the characteristics of a scenario--it is a story that describes how events unfold over time and includes vivid details that create a mental picture or vision of the future.) Write a scenario that describes this success story for Internet shopping.

In the failure scenario writing task, the title of the writing task was "Worst-Case Scenario: Internet Shopping is a Failure." Subjects were instructed as follows:

> Please think about a worst-case scenario in which Internet shopping will not succeed. (Recall the characteristics of a scenario--it is a story that describes

> how events unfold over time and includes vivid details that create a mental picture or vision of the future.) Write a scenario that describes this story of failure for Internet shopping.

Similarly, in the writing task for success reasons, the writing task was titled "Best-Case: Internet Shopping is a Success" and subjects were instructed as follows:

> Please think about reasons why Internet shopping will succeed. List as many reasons as you can to support the success of Internet shopping.

In the writing task for failure reasons, the writing task was titled "Worst-Case: Internet Shopping is a Failure" and subjects were instructed as follows:

> Please think about reasons why Internet shopping will not succeed. List as many reasons as you can to support the failure of Internet shopping.

Subjects then responded to a series of questions concerning the future of Internet shopping, including a final opinion, market share predictions over time, likelihood and confidence measures, recommendations for launch of on-line sales in various product/service categories, and predictions of attitudes of various population groups. Subjects also provided some background information (e.g., prior knowledge). Subjects were then presented with a set of pro and con reasons representing "another opinion about the future of Internet shopping" (which may be "helpful or not in forming your professional opinion").

Results and Discussion

The study predicted a two-way interaction between type of elaboration (scenario versus reasons) and order. Judgments were expected to be more biased toward an initial scenario than toward initial reasons generated. Results across all of the dependent measures are consistent with this hypothesis.

First, consider subjects' direct ratings of the likelihood of success and failure for Internet shopping (see Table 7). Subjects were asked to indicate whether success or failure best describes the future of Internet shopping and also to rate the likelihood of the reasons/scenarios that they wrote describing success and failure. A CATMOD analysis on the binary choice between success and failure indicates a significant two-way interaction of type and order of elaboration. Subjects predicted failure outcomes more often when their initial scenario supported failure or their second set of reasons supported failure (χ^2=4.40, p=.036). In addition, analyses of likelihood ratings also indicated a significant two-way interaction of type and order of elaboration ($F_{1,87}$=8.13, p=.005). Likelihood ratings reflect a primacy effect for scenarios and a recency effect for reasons. The recency effect in the reasons control groups rules out fatigue and demand as alternative explanations, whereas the primacy effect for scenarios supports the study's hypothesis.

This pattern of results is also found on several other less direct judgment measures displayed in Table 8. For example, subjects were asked to recommend whether a firm should launch on-line sales in a variety of product/service categories (e.g., lawyer services, housing, furniture, hardware, clothing, etc.). The number of launch recommendations appears to be driven by the two-way interaction of type and order of elaboration ($F_{1,88}$=3.86, p=.053).[9] Subjects were also asked to predict attitudes toward Internet shopping for a number of different groups (e.g., adults, teens, seniors, manufacturers, retailers, catalogers, etc.) as well as their personal attitude. A MANOVA of these predictions also revealed a significant two-way interaction of type and order of elaboration ($F_{1,87}$=6.48, p=.013). Overall, predictions were more favorable toward Internet shopping when subjects generated a success scenario initially or when subjects generated reasons for success more recently.[10]

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[9] A MANOVA of subjects' confidence across all of these recommendations also revealed a significant two-way interaction of type and order of elaboration ($F_{1,86}$=3.40, p=.069).

Some additional repeated dependent measures were taken and their analysis deserves special attention. First, consider the attitude measures that were taken at three points in time: initially (prior to elaboration), after the ordered elaboration task, and after presenting pro and con reasons to subjects. These attitude judgments can be used to construct two attitude change measures to examine whether changes in attitude are induced by the ordered elaboration tasks and by presenting pro and con information. A MANOVA of both attitude change measures indicates a significant two-way interaction of type of elaboration and order ($F_{1,88}$=4.97, p=.028).[11] As the results in Table 9 and Figure 3 indicate, attitude changes appear to depend upon both the type of elaboration and the order of elaboration. For reasons, there is a recency effect of the second set of reasons generated: attitudes become more negative over time in the pro-con order and more positive over time in the con-pro order. In contrast, for scenarios, there is a primacy effect of the initial scenario: attitudes become more positive over time in the best-case-worst-case order and more negative over time in the worst-case-best-case order. Although the interaction with repetition is not significant, it does appear at least directionally from the means that the two-way interaction becomes stronger over time ($F_{1,88}$=1.95, p=0.167). If this were the case, then it would suggest that presenting relatively evenhanded reasons can actually exacerbate the order-driven bias, rather than reducing it. In any case, there is no indication that presenting pro and con reasons was effective at reducing the bias in judgment.

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[10] For brevity's sake, these results are not reported separately in tables in this chapter. Results are available from the author upon request.

[11] In addition, there is a significant interaction of type of elaboration and the two attitude change measures ($F_{1,88}$=6.21, p=.015), but this must be interpreted in light of the two-way interaction of type of elaboration and order.

Second, consider predictions for market share, which were repeated both after the ordered elaboration task and after presenting pro and con information (see Table 10). Subjects were asked to estimate the percentage of all retail sales that will take place on the Internet in 2003, 2008, 2013, and 2018 and beyond. A MANOVA of these estimates indicates a significant four-way interaction of type of elaboration, order of elaboration, estimation period (cubic polynomial), and repeated judgment ($F_{1,83}$=4.63, p=.034). Follow-up analysis indicates that the three-way interaction (type X order X period^3) was not significant (although the means are directional) for market share judgments following initial elaboration but was significant following presentation of reasons ($F_{1,85}$=6.33, p=.014). As Figure 4 indicates, the slopes for market share over time are higher when the initial scenario described success (versus failure) and when the second set of reasons described success (versus failure).

An alternative analysis of this data examined the change in market share predictions with repeat judgment (see Table 10). A MANOVA indicated a marginal two-way interaction of type and order of elaboration ($F_{1,85}$=3.05, p=.085), qualified by an interaction with success estimation period ($F_{3,83}$=2.195, p=.095). Follow-up univariate analyses indicate that the two-way interaction has an effect on changes in market share for 2003 ($F_{1,85}$=2.99, p=.087), 2008 ($F_{1,85}$=4.00, p=.049), 2013 (F<1) and 2018 and beyond ($F_{1,85}$=3.58, p=.062). In other words, although initial market share judgments were not significantly affected by type and order of elaboration, repeated judgments after presenting reasons did reveal a significant bias that was driven by the primary scenario or recent reasons. Figure 5 is a graph of the *average* change across time periods. The results suggest that presenting pro and con evidence may exacerbate rather than correct bias in judgment and are also consistent with the direction of results for the attitude change measure.

Although this latter finding is not conclusive, it merits additional research. Even the failure of presented evidence to debias judgment is surprising.[12] Common sense suggests that people will base judgments on presented evidence; indeed, this is a basic premise of a great deal of research. On the one hand, a null effect could merely indicate a weak manipulation. For example, the presented evidence may have been perceived as weak or unconvincing, although this is not indicated by the ratings.[13] On the other hand, some prior research suggests that presented evidence can have unintended effects on judgment. For example, Lord, Ross, and Lepper

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[12] Such was the case in a study by Anderson and Sechler (1985), who found that subjects moderated their beliefs in a social theory somewhat but did not abandon them when exposed to contradictory data (e.g., zero relationship between risk-taking and fire-fighting). Their study is particular interesting because it forms the basis for, and contrasts with, Hirt and Markman's subsequent work demonstrating that multiple explanation debiases judgment.

[13] Pro evidence was rated 4.93 (1.32) on an unconvincing-convincing 1-7 scale. Con evidence was rated 4.66 (1.48). Ratings did not vary significantly by condition.

(1979) demonstrated biased assimilation of pro/con evidence presented to subjects and polarization of attitudes (toward the death penalty). Similarly, Miller, McHoskey, Bane, and Dowd (1993) found that biased assimilation of evidence and attitude polarization occurred especially among people holding extreme, versus moderate, attitudes. Perhaps presented evidence polarizes judgment if it enables subjects to focus on supportive evidence and ignore or discount opposing evidence. In a sense, evenhanded evidence contains further arguments that serve as grist for the mill and aid people in constructing explanations to support and bolster their initial attitude. However, Lord et al.'s result has not always been replicated (e.g., Anderson & Sechler 1986; for a review, see Kuhn & Lao 1996) and this study's evidence is not strong.

Overall, the results of this study provide considerable support for the notion that there are order effects in judgment for scenarios and reasons. First, primacy order effects were stronger for scenarios than reasons: generating an initial scenario influenced judgment more than generating initial reasons. In other words, scenarios lead to 'stickier priors' than reasons. Second, order effects endure despite counterfactual thinking; hence, judgments remained biased in the direction of an initial scenario generated, even after the generation of a counter-scenario. This susceptibility to order effects raises questions about the efficacy of scenario analysis, which relies on the generation of multiple alternative scenarios to improve forecasting. And, finally, I have presented some evidence that subsequent attempts at debiasing judgment via presentation of reasons may in fact exacerbate rather than eliminate bias. Certainly, there is no evidence that providing evenhanded facts eliminated the scenario-induced bias in judgment.

Table 7.
Study 1: success ratings after ordered elaboration for Internet shopping (IS).

Type of Elaboration	Order	# of Subjects predicting success	Likelihood that IS succeeds	Likelihood that IS fails
Reasons	Success-Failure	14 / 20 (70%)	4.76 (1.51)	3.48 (2.71)
Reasons	Failure-Success	19 / 21 (90%)	5.48 (1.12)	2.71 (1.38)
Scenarios	Success-Failure	22 / 25 (88%)	5.28 (1.14)	3.12 (1.20)
Scenarios	Failure-Success	18 / 25 (72%)	4.63 (1.47)	3.92 (1.56)

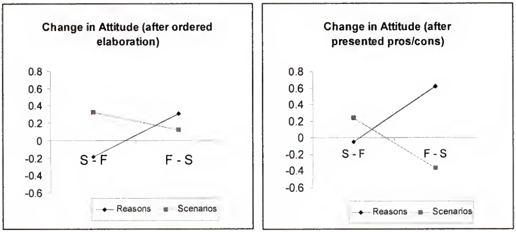
Table 8.
Study 1: indirect judgments after ordered elaboration for Internet shopping.

Type of Elaboration	Order	# of Launch Recommendations for Products/Services	Predictions of Others' Attitudes (averaged)
Reasons	Success-Failure	4.71 (2.08)	4.28 (0.72)
Reasons	Failure-Success	6.24 (1.14)	4.68 (0.74)
Scenarios	Success-Failure	5.40 (1.73)	4.63 (0.69)
Scenarios	Failure-Success	5.56 (1.56)	4.15 (0.96)

Table 9.
Study 1: attitudes toward Internet shopping before and after ordered elaboration and after presented evidence.

Type of Elaboration	Order	Initial Attitude	Attitude after Elaboration	Attitude after Presented Evidence	Difference after Elaboration	Difference after Presented Evidence
Reasons	Success-	4.33	4.14	4.29	-0.19	-0.05
	Failure	(1.24)	(1.46)	(1.55)	(1.25)	(1.02)
Reasons	Failure-	4.56	4.88	5.19	0.31	0.62*
	Success	(1.29)	(1.14)	(1.44)	(0.93)	(1.20)
Scenarios	Success-	4.56	4.88	4.80	0.32	0.24
	Failure	(1.12)	(1.05)	(1.04)	(0.90)	(0.72)
Scenarios	Failure-	4.72	4.84	4.36	0.12	-0.36
	Success	(1.21)	(1.43)	(1.60)	(1.42)	(1.55)

Note: * indicates change in attitude is significantly different from 0 (p=.017). However, the possibility of attitude drift implies that contrasts to zero are an inappropriate analysis.



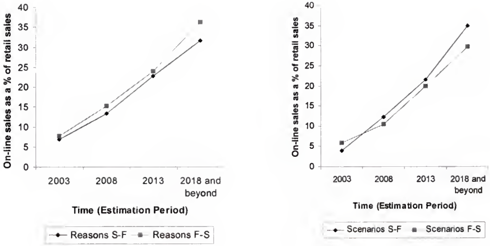
A B

Figure 3.
Study 1: change in attitude after ordered elaboration (A) and presented evidence (B).

Table 10.
Study 1: market share estimates for internet shopping after ordered elaboration and after presented evidence.

Type of Elaboration	Order	Market Share Estimates over Time (Measured after Ordered Elaboration)				
		2003	2008	2013	2018+	
Reasons	Success	8.27	14.74	22.96	34.33	
	-Failure	(12.44)	(18.04)	(25.63)	(34.03)	
Reasons	Failure-	6.29	14.65	24.19	37.67	
	Success	(8.89)	(14.41)	(22.68)	(29.84)	
Scenarios	Success	4.15	12.47	24.39	37.67	
	-Failure	(4.67)	(11.82)	(20.13)	(26.67)	
Scenarios	Failure-	6.52	14.41	25.48	36.02	
	Success	(5.84)	(11.94)	(21.65)	(28.64)	
		Market Share Estimates over Time (Measured after Presented Evidence)				
		2003	2008	2013	2018+	
Reasons	Success	6.87	13.29	22.65	31.64	
	-Failure	(11.74)	(17.04)	(25.76)	(33.15)	
Reasons	Failure-	7.67	15.22	23.88	36.26	
	Success	(10.04)	(13.61)	(19.54)	(27.37)	
Scenarios	Success	4.03	12.26	21.64	35.04	
	-Failure	(4.19)	(11.71)	(16.96)	(25.10)	
Scenarios	Failure-	5.84	10.44	19.99	29.74	
	Success	(6.34)	(8.30)	(18.03)	(27.18)	
		Change in MS 2003	Change in MS 2008	Change in MS 2013	Change in MS 2018 +	Average Change in MS
Reasons	Success	-1.4	-1.4	-0.3	-2.7	-1.4
	-Failure	(5.4)	(7.83)	(11.3)	(5.2)	(6.5)
Reasons	Failure-	1.4	0.6	-0.3	2.2	+0.9
	Success	(6.8)	(8.8)	(11.4)	(15.0)	(9.8)
Scenarios	Success	-0.1	-0.2	-2.7	-2.6	-1.4
	-Failure	(1.1)	(3.6)	(6.9)	(7.8)	(3.6)
Scenarios	Failure-	-0.7	-4.0	-5.5	-6.3	-4.1
	Success	(3.1)	(6.2)*	(9.2)*	(11.9)*	(6.6)*

Note: * indicates change is significantly different from 0 (p<.01). However, the possibility of attitude drift implies that contrasts to zero are an inappropriate analysis.



A

B

Figure 4.
Study 1: market share estimates for Internet shopping after presented evidence in the "ordered reasons" conditions (A) and "ordered scenarios" conditions (B).

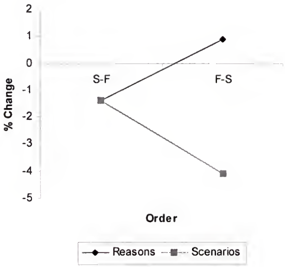


Figure 5.
Study 1: average change across presented evidence in market share estimates for Internet shopping.

CHAPTER 6
STUDY 2: SCENARIOS AND ACCOUNTABILITY

The previous studies suggest that counterfactual reasoning is ineffective at debiasing judgment induced by an initial scenario. Neither generated nor presented evenhanded reasons eliminated scenario effects; generated counter-scenarios also failed. In this study, I try a different approach and investigate whether accountability will debias judgment.

Popular wisdom suggests that accountability (expecting to have to justify one's opinions to others, a common feature of the business world) is sufficient to avoid many judgmental biases demonstrated in a laboratory setting. Some research suggests that accountability improves judgment by inducing "more integratively complex" thinking (i.e., analytic thought). In a number of studies, accountability has been found to induce evenhanded consideration of evidence (Tetlock 1983a) and reduce judgmental biases (Tetlock 1985; Tetlock & Kim 1987; Simonson & Nye 1992). For example, the order effect in generating pro and con evidence in a court case was reduced when people expected to justify their position to others (Tetlock 1983b). When Tetlock and Boettger (1994) showed people evidence of the risks and benefits of a new drug product, accountability led to risk aversion in a launch decision. However, subsequent research suggests that accountability is not a broad panacea for correcting errors in judgment (Simonson & Nye 1992; Tetlock &

Boettger 1989). For example, accountability has been shown to exacerbate confirmation bias in judgments of causal responsibility (Sanbonmatsu, Akimoto, & Biggs 1993). In an attempt to reconcile some of these conflicting findings, Tetlock, Skitka, and Boettger (1989) suggest that accountability demands can be accommodated in three ways: *bolstering* one's own position if already committed to it, *conforming* to known or suspected views of one's audience[14], and thinking in cognitively complex ways (*pre-emptive self-criticism*).

Lerner and Tetlock (1999) have recently synthesized prior research on accountability. Accountability is said to attenuate bias that "results from lack of effort, self-critical awareness of one's judgment process, or both," i.e., by prompting pre-emptive self-criticism (p.263). If so, one would expect accountability to enhance analytic thinking by motivating people to consider-the-opposite or multiple alternatives in expectation of having to justify their views. In contrast, accountability is said to amplify bias that "results from naïve use of normatively (but not obviously) irrelevant cues" (p.264). I have previously argued that scenarios incorporate imagination and story-telling, nonanalytic features or irrelevant cues that are self-generated and bias judgment. If so, accountability may exacerbate bias if it motivates people to elaborate more extensively using a nonanalytic thought process such as scenarios. Therefore, I propose that accountability will exacerbate overconfidence induced by scenarios; in the absence of scenarios, accountability will lead to conservatism.

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[14] In a field study, Brown (1999) finds that accountability leads managers to 'policy bolster' by conforming to socially acceptable decision strategies. When asked to account for their views, analytically-oriented professionals increased their reliance on market research data; judgment-oriented creative staff decreased their reliance on the same data.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of five cells in a 2 (Scenario) x 2 (Accountability) between-subjects design (with counter-balancing). A total of 190 subjects participated, with the number in each cell ranging from 25 to 33.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. Subjects worked through the materials at their own pace. Subjects played the role of a new product manager and were asked to provide their "gut reaction" to several new product ideas. Some subjects then imagined and wrote a scenario describing the future success of the target new product (the Electronic Book). The scenario manipulation was incorporated into the role-playing task as follows:

> This stage of the new product process asks you to imagine the idea's potential success... In doing so, you may find that you need to fill in some details regarding the product, how it might be used, its features, and its appeal to customers. For example, what would it take for this new product to be successful? What would it be like to use this new product? What would the world be like if this new product were widely available and popular? Use your imagination to form a vision of the future success of the EBook.

In the scenario conditions, subjects received an accountability manipulation before the scenario generation task, before judging, or not at all; in the no-scenario condition, subjects received an accountability manipulation either before judging or not at all. The wording of the accountability manipulation was also consistent with the role-playing task:

> You may also be called upon to explain or justify your position to 'senior management' (in a brief interview with the experimenter at the end of this session).

A question at the end of the task asked subjects to report whether they recalled this manipulation and also advised subjects that interviews would only occur if time permitted at the end of the session (to maintain the fiction of accountability). All subjects then performed a "feasibility assessment" and generated pro and con reasons regarding the success of this new product idea. As in prior studies, subjects then made a series of predictions that included probability of success over time, market share over time, launch recommendation, and confidence.

Results and Discussion

Manipulation Checks

At the beginning of the survey, a screening question was included that asked subjects to indicate their initial reaction to the new product idea. Subjects who did not indicate a positive reaction were eliminated from subsequent analyses[15] (reflecting the real-world notion that managers consider only new product ideas that seem plausible on the surface). At the end of the survey, subjects were asked if they recalled the accountability manipulation. The accountability manipulation had the predicted effect on responses to the manipulation check (χ^2=28.40, p=.0001).[16]

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[15] Thirty-eight subjects indicated a negative initial reaction and were eliminated from all analyses. In addition, probability estimates must logically increase over time; probability estimates from 22 subjects that did not meet this criteria were omitted. The distribution of eliminations across conditions did not appear to follow any pattern for eliminations based on initial reactions. For probability logic checks, eliminations were less evenly distributed. However, the key analysis is also conducted on other dependent variables with these subjects included. Because the results are consistent, the eliminations do not bias the conclusions. Unless otherwise indicated, any other differences in cell sizes during hypothesis testing are due to missing response data.

Dependent Variables

An initial analysis treated the data as a 2 (Scenario) x 2 (Accountability) design and combined conditions in which subjects received the accountability manipulation before or after imagining the scenario.[17] A MANOVA of predictions for probability of success and market share over time as a function of scenario generation and accountability was conducted. This analysis revealed a significant two-way interaction ($F_{1,123}$=3.55, p=.062).[18] This finding is central, because it indicates that the overall effect of accountability on judgment depends upon whether subjects generated scenarios. To explore this result further, MANOVAs for each prediction variable were also conducted. The interactions were found to be significant for both probability of success over time ($F_{1,124}$=3.31, p=.071) and market share ($F_{1,144}$=3.46, p=.065). Follow-up tests of the simple effects of accountability were also examined for each prediction variable. For probability of success estimates, accountability led to more optimistic judgments when scenarios were generated ($F_{1,124}$=4.58, p=.034); otherwise, accountability had no effect (F<1). For market share estimates, accountability led to marginally more conservative estimates when scenarios were not generated ($F_{1,144}$=2.63, p=.107); otherwise, accountability had no effect (F<1). The overall pattern of means is consistent: without scenarios, accountability led to more conservative judgments; with scenario generation, accountability exacerbated optimism. This pattern is evident in the slopes of each of the lines, or more simply the average prediction mean across all periods (see Tables 11 and 12 and Figure 6).

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[16] Of those students who received the accountability manipulation, 83% correctly recalled it; 57% of students who did not receive the accountability manipulation check correctly responded that they did not. (Incorrect recall may arise from an affirmative response bias. The response errors, if reflecting a genuine feeling of heightened accountability, would actually weaken the chance of finding a significant effect of accountability by reducing the strength of the accountability manipulation.) All subjects were retained in subsequent analyses.

[17] A MANOVA of predictions for probability of success and market share over time was conducted as a function of when subjects received the accountability manipulation in the scenario condition (order was counter-balanced). In all cases, the order effect was non-significant, which justifies collapsing across order.

[18] The two-way accountability X scenario interaction was qualified by an interaction with time and the prediction variable (cubic-time $F_{1,123}$=3.70, p=.057). This higher-order interaction reflects the notion that the strength of the two-way interaction of scenario and accountability depends upon the prediction variable and the time period. Practically speaking, the probability and market share predictions were cumulative and non-cumulative (respectively), so it would not be surprising if a 'levelling off' occurred in more distant time periods for market share but not probability. In fact, there is a cubic-time interaction for market share ($F_{1,144}$=3.67, p=.057) but not for probability of success ($F_{1,124}$=2.00, p=.160).

In addition to these results on the key dependent measures, analyses of several related dependent variables provide additional support. For example, subjects were asked to predict how long it would take "for the first Electronic Book to be launched nationally in the USA" and "for over 1 million people to own an Electronic Book." There is a significant two-way interaction of accountability and scenarios ($F_{1,146}$=3.92, p=.050), qualified by an interaction with the prediction variable ($F_{1,146}$=4.909, p=.028).[19] As the means in Table 13 indicate, subjects who did not generate scenarios predicted a longer time period if held accountable (a conservatism bias); in contrast, subjects who did generate scenarios predicted a shorter time period under accountability (an optimism bias). The effect is particularly pronounced for predictions regarding the 1 million ownership mark, compared to launch.

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[19] Follow-up univariate analyses reveal non-significant effects for time-to-launch (F<1) but significant effects for time-to-1-million-ownership ($F_{1,146}$=4.99, p=.027). In simple effects tests, accountability led to more conservative predictions when scenarios were not generated ($F_{1,146}$=3.69, p=.057); the effect of accountability was non-significant when scenarios were generated ($F_{1,146}$=1.39, p=.241).

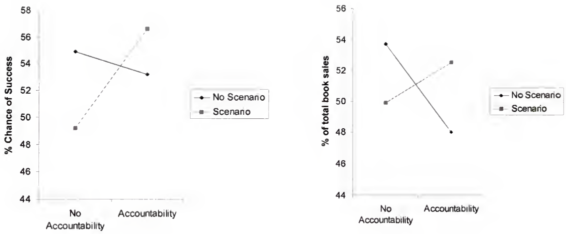
Overall, these results suggest that the accountability of real-world managers may not improve nonanalytic judgment. It can be viewed as a relatively conservative test, inasmuch as the scenario task was only a few minutes long and all subjects also generated evenhanded pro and con reasons prior to judgment. Consistent with all of the studies thus far, generating evenhanded reasons seems insufficient to improve judgment after nonanalytic thinking. Worse still, accountability may exacerbate a scenario-induced bias by increasing its resistance to evenhanded reasoning. Perhaps accountability motivated subjects to think harder, not smarter, and try to 'tell a better story'. In doing so, generating a more elaborate scenario may have exacerbated scenario-induced bias, consistent with the findings of Pilot Study 2. Interestingly, Sieck, Quinn, and Schooler (1999) recently demonstrated that justification leads to polarization of analogy judgments. These findings are particularly disconcerting inasmuch as accountability is generally thought to improve judgment by inducing evenhanded and analytic thought. In contrast, accountability appears to exacerbate one-sidedness in nonanalytic thinking.

Table 11.
Study 2: probability of success estimates over time as a function of scenarios and accountability for electronic books.

Scenario	Account-ability	N	Next 5 years	Next 10 years	Next 20 years	Beyond 20 years	Average
No Scenario	Not Accountable	24	19.71 (11.79)	44.13 (17.78)	69.17 (15.16)	88.96 (12.14)	54.86 (10.79)
No Scenario	Accountable	27	20.76 (18.86)	40.87 (20.92)	65.22 (20.68)	85.93 (15.13)	53.19 (17.04)
Scenario	Not Accountable	26	13.38 (10.04)	35.04 (15.85)	64.38 (20.37)	83.81 (16.02)	49.15 (13.32)
Scenario	Accountable	52	21.06 (19.63)	44.52 (21.97)	68.99 (20.00)	91.66 (10.60)	56.56 (15.16)

Table 12.
Study 2: market share estimates over time as a function of scenarios and accountability for electronic books.

Scenario	Account-ability	N	2000-2005	2005-2010	2010-2020	Beyond 2020	Average
No Scenario	Not Accountable	25	23.00 (14.07)	43.60 (16.99)	70.20 (20.13)	80.80 (23.83)	53.70 (11.86)
No Scenario	Accountable	30	28.07 (29.93)	38.33 (21.73)	53.50 (21.98)	69.50 (29.43)	47.98 (17.26)
Scenario	Not Accountable	32	24.09 (19.29)	41.87 (22.17)	57.19 (22.75)	73.36 (28.37)	49.94 (15.44)
Scenario	Accountable	62	27.69 (24.42)	43.90 (20.51)	60.77 (22.55)	77.63 (29.21)	52.50 (16.52)



A

B

Figure 6.
Study 2: average probability of success estimates (A) and market share (B) as a function of scenarios and accountability for electronic books.

Table 13.
Study 2: predictions for launch and target sales as a function of scenarios and accountability for electronic books.

Scenario	Account-ability	N	# of years until national launch	# of years until 1-million own Ebooks
No Scenario	Not Accountable	25	4.02 (2.96)	9.20 (5.43)
No Scenario	Accountable	30	4.83 (4.13)	13.13 (10.30)
Scenario	Not Accountable	32	4.41 (4.62)	11.76 (7.75)
Scenario	Accountable	62	3.99 (3.48)	9.75 (6.84)

CHAPTER 7
ANALOGIES

Analogies are another potential source of bias in judgment and will be the focus of the second part of this dissertation. An analogy, of course, is a comparison of two objects on certain dimensions, usually for the purpose of inference making. Reasoning by analogy about a target appears to consist of several steps: access or retrieval of a base analog, mapping of the base and target, inference making, and perhaps higher order learning (e.g., Holyoak & Thagard 1997; Gregan-Paxton & Roedder John 1997). Prior research on analogies has focused on factors that influence each step of reasoning by analogy. Evidence suggests that analogical retrieval and transfer is influenced by structural relations, surface details, problem context and goals, although their relative importance is a matter of continued debate (for reviews, see Reeves & Weisberg 1994; Gregan-Paxton & Roedder John 1997). The influence of structural relations reflects causal reasoning and analytic thinking; the influence of surface details, similarity-based inferences, and other salient non-causal information reflects nonanalytic thinking.

Prior research suggests that the retrieval stage may be especially susceptible to surface and contextual details that serve as cues to retrieval. For example, subjects are more likely to spontaneously retrieve a close analogy from a medical domain than a remote analogy from a military domain in generating a solution to a medical problem (Gick & Holyoak 1980). As with priming effects, people may be unaware of the analogy's effect on judgment (Schunn & Dunbar 1996). The effects of superficial similarities in reasoning by analogy can have dramatic effects on judgment. For example, people are more likely to recommend direct military intervention in a conflict scenario when irrelevant details (e.g., refugees fleeing in boxcars versus small boats) cue an analogy to World War II versus Vietnam (Gilovich 1981). Similarly, people who judge the likely success of a rookie football player are biased by irrelevant information (e.g., hometown) in an analogy to a successful professional player. Gilovich's result is particularly notable because the irrelevant analogy appears to bias judgment for both experts and novices, although some researchers speculate that these effects may be amplified for novices (e.g., Gregan-Paxton & Roedder John 1997). In these examples, the effect of surface details originate during the retrieval stage and carried through mapping and inference making. Holyoak and Thagard (1995a) note: "Too often people...make use of superficially appealing analogies to highly familiar source analogs, which actually miss most of the important causal relations in the target... People sometimes latch on to a salient analogy and focus on it to the neglect of deeper thought about the relevant problem, decision, or explanation."

Prior research suggests that people do try to be rational when reasoning by analogy. For example, people prefer to generate analogical mappings that honor basic coherence constraints (Spellman & Holyoak 1992). People also prefer to make inferences based on systematic correspondence between the target and the analogy (Markman 1997). Thus, knowledge in the analogy's domain influences inference making in the target domain. For example, in a study by Spellman and Holyoak (1992), people's mappings between World War II and the Persian Gulf War were influenced by their knowledge of World War II; inferences about the Persian Gulf War were altered by emphasizing different aspects of World War II. Mappings are also influenced by goals. For example, subjects tend to resolve ambiguous mappings in a manner that favors their goals (Spellman & Holyoak 1996).

Thus, reasoning by analogy is a fallible process. Inferences are typically generated by aligning the base and target and copying relations from the base to the target. The relations are known to be true about the base but are not necessarily true about the target. However, the analogy lends support or plausibility to these target inferences. For example, people judge a target explanation of an event as more likely when a consistent analogy is provided (Read & Cesa 1991). In addition, the analogy also emphasizes some dimensions of the problem at the expense of others by focusing attention on the mapped rather than unmapped aspects of the target (Gentner & Markman 1997). As a result, even appropriate analogies (based on relevant dimensions) can be over-applied, leading to neglect of other dimensions or inappropriate inferences. In other words, the analogy may serve as a mental model or focal hypothesis that drives elaboration in a relatively one-sided manner. Zaltman (1997) argues more generally that metaphor guides attention and perception, elicits hidden knowledge and serves as the "engine of imagination." Analogies may be particular enticing in uncertain or complex judgments because they provide a coherent interpretation of a focal problem, i.e., local consistency at the expense of global consistency (Klayman & Schoemaker 1993). If this is the case, then elaboration may lead to one-sided reasoning by analogy, thereby reinforcing the analogy and polarizing judgment. Evidence for reinforcement was found in a study by Sieck, Quinn, and Schooler (1999). Justification (a special form of elaboration) polarized analogy judgments, and subjects appeared to engage in a one-sided search for commonalities rather than differences in justifying their judgments.

In the absence of hard data and causal evidence, new products judgments may be especially susceptible to reasoning by analogy. The question then turns to whether the analogy is appropriate. Presumably analogies based on superficial similarity, either surface or global, are not necessarily useful guides to judgment. Even if the analogy is relevant on some dimensions, elaboration may lead to inappropriate inference making on other dimensions. Based on Gilovich's strong evidence, as well as arguments drawn from the literature on reasoning by analogy, I propose that analogies (like scenarios) can bias judgment. This notion underlies Proposition 1, and my research is designed to provide evidence of analogy effects in a new product context. Furthermore, analogies may be important when the effectiveness of analytic tools are examined. I am unaware of any direct findings in prior work on analogies that test whether their effects can be eliminated by analytic thinking. The resistance of analogies to subsequent analytic thought is the basis of Proposition 2 and will be investigated in a series of studies.

CHAPTER 8
PILOT STUDY 3: ANALOGIES AND ELABORATION

The first objective of this study is to provide an initial demonstration of the effects of analogies and their resistance to subsequent analytic thinking. The study is also designed to investigate the effects of elaboration on analogy-induced biases.

As previously discussed, some research suggests that analogy-induced biases may be the result of relatively superficial reasoning by analogy (Gilovich 1981). On the other hand, some researchers suggest that people prefer analogical mappings that are rational and based more on deep structure than surface similarities, although their relative importance is a matter of continued debate (Reeves & Weisberg 1994). The (unstated) implication seems to be that if people were to elaborate more deeply on an analogy, they might recognize that its similarity is superficial and then correct for the bias. At the very least, additional thought should lead to consideration of the deeper structure of the problem and induce more analytic thinking. If this is the case, then elaboration may reduce the effects of a superficial analogy.

On the other hand, Sieck, Quinn, and Schooler (1999) provide some evidence that subjects weight surface similarity more heavily when asked to justify their judgments (instead of shifting to heavier weighting of the analogy's deep structure). Moreover, judgments of the analogy polarized as a result of justification, and subjects appeared to consider commonalities rather than differences in a one-sided manner.[20] I have previously argued that analogies lead to relatively one-sided elaboration and the development of a schema that reinforces the initial analogy. Insofar as reasoning by analogy tends to reinforce the analogy, it could account for the persistence of analogy-induced biases over time. This hypothesis is consistent with prior research on polarization (e.g., Millar & Tesser 1986), which suggests that additional thought may serve to bolster a schema and polarize judgments. Similarly, adopting and elaborating an analogy may also induce directional thought and lead to polarized judgment.

This study is designed to investigate the effects of elaboration on analogy-induced biases. Recall that Pilot Study 2 and Study 2 investigated the effects of elaboration (directed and under accountability) on scenario-induced biases. This study provided subjects with an analogy followed by a relatively neutral elaboration instruction. Doing so provided a relatively stringent test of the effects of elaboration on judgment because subjects were free to elaborate in an evenhanded manner if they so choose. In addition, subjects were instructed to generate evenhanded reasons in an attempt to induce analytic thinking prior to judgment. Thus, ample opportunity for elaboration to reduce the superficial effects of an analogy was included in the study's design in order to address its hypotheses. That is, an initial analogy biases judgment and subsequent elaboration (via a neutral scenario and evenhanded reasons) does not eliminate its effects.

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[20] My interest lies in the analogy's effect on forecasts, rather than judgments of the analogy itself.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of four cells in a 2 (Scenario versus not) x 2 (Analogy: positive/negative) x 2 (Repeat predictions: before and after pro/con generation) mixed design. A total of 109 subjects participated, with the number in each cell ranging from 20 to 31.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. Subjects worked through the materials at their own pace. The survey asked subjects to play the role of a business executive and think about the future of Electronic Books. Subjects were given some background information on the EBook and were asked to give an initial professional opinion. Subjects were then given an analogy to the success/fast growth of cellular phones or the failure/slow growth of videophones. In the success analogy condition, subjects were instructed as follows:

> Please think about the future of Electronic Books. In doing so, you may find it helpful to consider the case of cellular telephones since some people believe that the Ebook could be as successful in the marketplace as cellular phones.

In the failure analogy condition, subjects received the following instruction:

> Please think about the future of Electronic Books. In doing so, you may find it helpful to consider the case of video telephones since some people believe that the EBook could be as unsuccessful in the marketplace as videophones.

Subjects were also provided with numerical information and a graph of cumulative sales over time reflecting 25% penetration over 10 years for cellular phones in the USA (in the success analogy conditions) or less than 1% penetration over 10 years for videophones in the USA (in the failure analogy conditions).

Some subjects then generated a scenario describing the future of Ebooks. The scenario instructions were intended to be neutral:

> Please take some time to think about a scenario describing the future of Electronic Books. (Recall the characteristics of a scenario – it is a story that describes how events unfold over time and includes vivid details that create a mental picture or vision of the future.) Write a scenario that describes the future of the EBook.

Other subjects proceeded to the next section of the booklet.

Subjects then responded to a series of questions concerning the future of EBooks, including their opinion, probability of success of EBooks, adoption curve predictions over time, and likelihood and confidence measures (time 1). Subjects then generated pro and con reasons for the future of EBooks and responded to several measures again (time 2).

Results and Discussion

This study investigates whether an analogy biases judgment and whether elaboration (either via scenario generation or pro/con generation) can effectively debias judgment. A main effect of the analogy on judgment would indicate that the analogy induced bias. Depending on when the dependent measure is taken, it is also possible to test for the effectiveness of elaboration in correcting for this bias.

First, the results consistently demonstrate an analogy-induced bias. There is a significant main effect of the analogy on judgments made at time 1 (see Table 14). Subjects who were given an initial negative analogy changed their professional opinion in a more negative direction than subjects who were given a positive analogy ($F_{1,103}$=6.92, p=.0098). In addition, the positive analogy led to more favorable judgments of probability of success ($F_{1,104}$=6.42, p=.013), increased choice of success over failure as best describing the future of EBooks (χ^2=4.73, p=.030), and greater confidence in the success of EBooks ($F_{1,104}$=4.51, p=.036). The difference in likelihood ratings given to best-case and worst case future scenarios was smaller when subjects were given negative versus positive analogies ($F_{1,100}$=3.07, p=.083). Furthermore, subjects who were given a positive analogy were more likely to choose an adoption curve with the most rapid growth as best fitting the future of the EBook (χ^2=3.79, p=.052).

Whether subjects elaborated via scenarios had no effect on any of the judgments described above (both main effect of scenarios and interaction with analogies are non-significant for all prediction variables). These consistent findings[21] suggest that an initial analogy biases judgment and that elaborating on a future scenario does not undo the biasing effects of the initial analogy. There is no evidence that scenario elaboration exacerbates the initial bias.

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[21] Only one result deviates from this finding. There are no significant effects on predictions for the adoption curve (percentage of people who have adopted the Ebook in 2003, 2008, 2013 and beyond) made by subjects at time 1, and the direction of the means is not entirely consistent. (The slope is directionally steeper for the positive versus negative analogy in the no-scenario condition, but not in the scenario condition.) This finding is discussed in more detail when I examine the effects of generating pro and con reasons on repeated judgments.

Given the repeated measures design, it is also possible to investigate the effects of generating pro and con reasons on judgments that have been biased by an initial analogy. After elaborating via a scenario and then via generating pro and con reasons, the positive analogy still led to more favorable final judgments of probability of success than the negative analogy (see Table 15). Indeed, a MANOVA of both time 1 and time 2 probability judgments reveals a significant main effect of analogy ($F_{1,104}$=4.63, p=.034) and a non-significant interaction with repeated judgment, which suggests that the pro-con generation did not improve judgment on this prediction variable (all changes are non-significant). Similarly, subjects who were given an initial negative analogy still changed their opinion in a more negative direction than subjects who were given a positive analogy ($F_{1,103}$=3.42, p=.067). Again, a MANOVA of both attitude change variables (constructed from initial, time 1 and time 2 judgments; see Table 16 and Figure 7) reveals a significant main effect of analogy ($F_{1,103}$=6.43, p=.013) and a non-significant interaction with repeated judgment. Thus, there is no evidence that generating pro and con reasons had any debiasing effect on judgment.

Interestingly, there appears to be some evidence that the pro-con generation task may have exacerbated the analogy-induced bias. Predictions at time 2 for the adoption curve reveal a significant interaction of the analogy with time (quadratic $F_{1,104}$=7.04, p=.009). This interaction reflects predictions of slower growth by subjects given the negative versus positive analogy, as illustrated in Figure 8. However, this same analysis for predictions at time 1 revealed no significant effects.

A MANOVA of predictions at time 1 and time 2 reveals an overall significant interaction of analogy and repetition and estimation period (quadratic polynomial $F_{1,104}$=6.92, p=.0098). This interaction indicates that subjects change their predictions after generating pro and con reasons in the direction favored by the initial analogy, i.e., toward slower growth if given a negative analogy.

Another way of examining the effect of generating pro and con reasons is to analyze the change in adoption predictions from time 1 to time 2 (see Table 17). A MANOVA of these change measures, constructed by calculating the difference between time 1 and time 2 judgments, reveals that the effect of the analogy depends on the estimation period ($F_{3,102}$=3.263, p=.025). Follow-up univariate analyses of the changes for each estimation period indicate that the effects of the analogy are primarily due to its impact on the change in prediction for the year 2008 ($F_{1,104}$=5.73, p=.019). This is consistent with the quadratic interaction above: after generating pro and con reasons, subjects given a negative analogy lowered their estimates for the year 2008, thereby predicting a 'slower growth' adoption curve, compared to subjects given a positive analogy. Thus, the attempt to debias judgment by asking subjects to generate pro and con reasons did not improve judgment and perhaps may have exacerbated the analogy-induced bias in some of these forecasts. This result provides partial support for the exacerbating effects of elaboration, occuring not during initial elaboration via the scenario but instead between time 1 and time 2 (across the additional questions and evenhanded reason generation task).

Overall, the results of this study provide support for the hypotheses. Judgments were biased by an initial analogy. This result is consistent with prior research on the robust phenomenon of anchoring (e.g., Wilson, Houston, Etling, & Brekke 1996), a distinction that I will address in the next study. Moreover, the effects of an analogy endured despite subsequent elaboration. Elaboration via a scenario did not reduce the biasing effects of the initial analogy, which suggests that deeper processing of an analogy is not necessarily more analytic. On the other hand, there was little support for the notion that elaboration exacerbates bias (because the scenario effect was non-significant). In addition, elaboration via evenhanded reasoning did not eliminate the analogy-induced bias, and there was even some evidence suggesting that judgment polarized after pro/con reason generation. Due to the experimental design, however, it is unclear whether polarization occurred during the evenhanded reasoning task or during questioning. For example, prior research has demonstrated that witness' confidence in their testimony is susceptible to influence by post-event questioning (Shaw 1996). In this study, changes in judgment from time 1 to time 2 could have arisen from intervening questions; insofar as those questions prompt elaboration and inference-making driven by the initial analogy, then bias in judgment may be exacerbated. Certainly, there was no evidence that the effect of the initial analogy was eliminated, or even reduced, by evenhanded reasoning (which supports the main hypothesis of this study).

Table 14.
Pilot study 3: predictions for future of electronic books (at time 1) as a function of analogy and scenario elaboration.

Scenario	Analogy	Subjects choosing graph with fastest growth	Subjects choosing "Best-Case" future	Confidence in Choice of Best-Case future	Difference in Scenario Likelihood Ratings (Best-Worst Case)
No Scenario	Negative	5 / 23 (22%)	16 / 23 (70%)	55.83 (30.55)	1.26 (2.83)
No Scenario	Positive	9 / 20 (45%)	17 / 20 (85%)	67.00 (25.46)	2.05 (2.74)
Scenario	Negative	10 / 33 (40%)	25 / 33 (76%)	64.85 (20.48)	1.58 (3.04)
Scenario	Positive	14 / 32 (44%)	30 / 32 (94%)	72.34 (13.44)	2.68 (2.12)

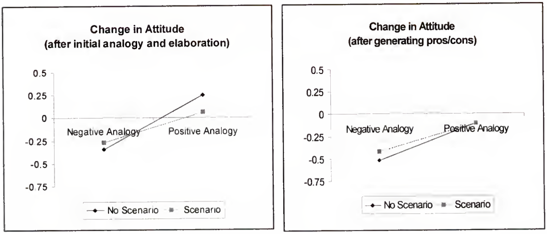
Table 15.
Pilot study 3: probability of success of electronic books as a function of analogy and scenario elaboration before and after reason generation.

Scenario	Analogy	N	Probability at time 1 (after analogy and scenario manipulation)	Probability at time 2 (after generating pros and cons)
No Scenario	Negative	23	54.35 (21.71)	53.91 (25.72)
No Scenario	Positive	20	66.50 (14.24)	66.00 (20.10)
Scenario	Negative	33	60.91 (17.74)	63.64 (18.00)
Scenario	Positive	32	65.94 (14.56)	64.38 (17.22)

Table 16.
Pilot study 3: attitude toward electronic books as a function of analogy and scenario elaboration before and after reason generation.

Scenario	Analogy	N	Initial Attitude	Attitude At time 1	Attitude at time 2	Diff 1	Diff 2
No Scenario	Negative	23	5.04 (1.46)	4.69 (1.52)	4.52 (1.56)	-0.34 (0.57) p=.067	-0.52 (1.31) p=.016
No Scenario	Positive	20	4.95 (1.14)	5.20 (1.15)	4.85 (1.35)	+0.25 (0.79) n.s.	-0.10 (0.85) n.s.
Scenario	Negative	33	5.30 (1.02)	5.03 (1.33)	4.88 (1.19)	-0.27 (1.13) p=.085	-0.42 (1.03) p=.019
Scenario	Positive	31	5.03 (1.02)	5.10 (1.04)	4.94 (0.96)	+0.06 (0.89) n.s.	-0.10 (0.87) n.s.

Note: Diff 1 (Diff 2) is the difference between the attitude at time 1 (time 2) and the initial attitude. P-values are reported for testing whether differences are non-zero. However, the possibility of drift implies that contrasts to zero are an inappropriate analysis.



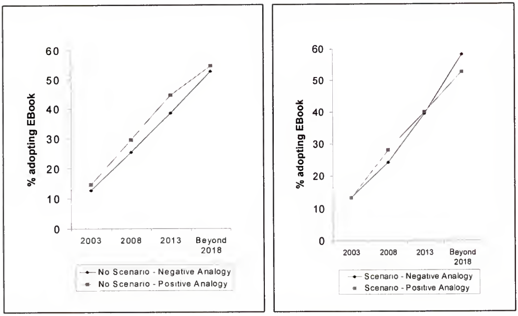
A B

Figure 7.
Pilot study 3: attitude change toward electronic books after initial analogy and scenario elaboration (A) and after reason generation (B)

Table 17.
Pilot study 3: adoption curve predictions for electronic books as a function of analogy and scenario elaboration before and after reason generation.

Scenario	Analogy	N	Adoption Curve Estimates Before Generating Reasons (Time 1)			
			2003	2008	2013	Beyond 2018
No Scenario	Negative	23	16.52	29.65	41.61	54.61
			(18.21)	(23.11)	(27.59)	(30.91)
No Scenario	Positive	20	14.56	29.65	45.10	59.65
			(11.01)	(22.00)	(29.02)	(34.15)
Scenario	Negative	33	15.88	33.03	49.36	65.58
			(15.88)	(19.90)	(25.61)	(28.52)
Scenario	Positive	32	14.59	29.19	45.16	58.47
			(14.59)	(21.29)	(26.93)	(29.46)
			Adoption Curve Estimates After Generating Reasons (Time 2)			
No Scenario	Negative	23	12.70	25.43	38.52	52.70
			(11.00)	(19.76)	(26.59)	(31.45)
No Scenario	Positive	20	14.50	29.50	44.65	54.55
			(9.56)	(20.03)	(29.42)	(34.14)
Scenario	Negative	33	13.47	24.29	39.45	58.33
			(9.34)	(14.20)	(20.89)	(28.11)
Scenario	Positive	32	13.22	28.09	39.88	52.72
			(11.36)	(19.80)	(25.31)	(28.56)
			Change in Adoption Curve Estimates with Repeated Judgment			
No Scenario	Negative	23	-3.83	-4.22	-3.09	-1.91
			(9.97)	(9.08)	(6.95)	(8.51)
			p=.027	p=.11	n.s.	n.s.
No Scenario	Positive	20	-0.05	-0.15	-0.45	-5.10
			(5.91)	(13.44)	(18.41)	(12.49)
			n.s.	n.s.	n.s.	p=.079
Scenario	Negative	33	-2.41	-8.74	-9.91	-7.24
			(6.74)	(14.66)	(17.19)	(14.97)
			p=.094	p=.0001	p=.0001	p=.002
Scenario	Positive	32	-1.38	-1.09	-5.28	-5.75
			(9.30)	(11.28)	(11.24)	(13.21)
			n.s.	n.s.	p=.037	p=.013

Note: P-values indicate whether change is significantly different from 0. However, the possibility of drift implies that contrasts to zero are an inappropriate analysis.



A B

Figure 8.
Pilot study 3: market share estimates after generating reasons for electronic books with (B) or without (A) scenario elaboration.

CHAPTER 9

PILOT STUDY 4: ANALOGY EFFECTS IN A MORE CONSERVATIVE TEST

The previous pilot demonstrated persistent effects of analogies. Subjects given an analogy to a successful product were more optimistic about a new product than subjects given an analogy to an unsuccessful product. This difference endured despite subsequent elaboration.

Pilot Study 4 serves as a more conservative test of analogy effects on judgment. In this study, some subjects were provided with an analogy; others were not. All subjects received the same information about the target new product. Subjects then elaborated by generating a scenario or reasons. In addition, subjects were presented with evenhanded evidence to consider. Thus, ample opportunity for elaboration to reduce the superficial effects of an analogy was included in the study's design in order to address its hypotheses. That is, an initial analogy influences judgment and subsequent elaboration and presented reasons does not eliminate its effects.

This study can be viewed as a more conservative test of analogy effects for several reasons. First, the analogy did not include an explicit numerical anchor. (It is difficult, if not impossible, to completely eliminate anchoring effects inasmuch as analogies can be viewed as sources of explicit or implicit anchors. Of course, it also can be argued that anchoring is not an explanation at all, but merely a description of the effect.) With this study design, the effect of the analogy was examined through comparison to a control group for which no analogy is provided (again, a more conservative test). The pro/con *generation* task of the previous pilot was replaced with pro/con *presentation*. Presentation of evenhanded evidence can be viewed as a stronger attempt to neutralize analogy effects because it reduces potential output interference effects that inhibit the generation of non-supportive evidence by subjects.

In addition, this study included another opportunity for analytic thinking through the use of a decomposition task. A more complete discussion of decomposition is reserved for study 4. For now, readers may view the decomposition task as simply another proposed method for inducing analytic thinking by prompting subjects to think about key drivers of new product success. Subjects were instructed to use a computational algorithm to estimate market share by combining their estimates for awareness, trial, liking and switching. Some subjects were asked to further elaborate on one component of the decompositional task. As before, I hypothesize that analytic thinking (in this case prompted by the decomposition task) fails to eliminate the effect of an initial analogy on judgment.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of 12 cells in a 3 (Type of elaboration: scenario, reasons, scenario + reasons) x 2 (Analogy: positive/none) x 2 (Type of decomposition) between-subjects design, with judgments taken at three points in time (after initial elaboration, after presented evidence, and after decomposition). A total of 101 subjects participated, with the number in each cell ranging from 7 to 11. (This small cell size is offset by planned analyses that collapse across selected factors.)

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. Subjects worked through the materials at their own pace. The survey asked subjects to assume the role of a manager at Procter & Gamble and think about the future of olestra (a fat-replacement food additive). Subjects were given some background information on olestra. Some subjects were cued to think about an analogous product (aspartame); others were not. The aspartame cue was intended to provide a positive analogy for subjects thinking about olestra. Specifically, subjects in the analogy conditions were told:

> You have just read a report that gives you some background information about olestra. Now, please take a few moments to think about the future of olestra. As you think about olestra, you may find it helpful to consider other food additives in the marketplace, such as aspartame (better known by its brand name NutraSweet).

Subjects in the no-analogy condition were told:

> You have just read a report that gives you some background information about olestra. Now, please take a few moments to think about the future of olestra.

All subjects were then instructed to elaborate on the future of this new product by generating a scenario, generating reasons, or generating a scenario followed by reasons. The scenario and reasons instructions were designed to be neutral. In the scenario conditions, subjects were told:

> We are particularly interested in what you think the future will be like for this new product. What will olestra's future be like? How will it unfold? What events will occur? How will things turn out for olestra? Please think about a scenario that describes olestra's future. (A scenario is a story that describes how events unfold over time and includes vivid details that create a mental picture or vision of the future.)

In the reasons conditions, subjects were told:

> We are particularly interested in what you think the future will be like for this new product. What will olestra's future be like? Why? What factors will influence olestra's future? Please think about reasons for and against the success of this new product.

In the scenario + reasons conditions, subjects received both instructions.

All subjects then predicted the future of olestra by responding to a series of questions, including binary choice measures and confidence, success ratings, and adoption estimates over time. The first booklet was then removed, and subjects were given a second booklet with the following instructions. "Now, please take a few minutes to consider another opinion about olestra's future. You may or may not find this information helpful in forming your own professional opinion." The information consisted of eight "possible reasons for olestra's success" and eight "possible reasons for olestra's failure," designed to provided relatively balanced and mixed evidence. Subjects repeated their judgments of olestra's future and rated the information and its impact on their judgment.

Following the presented evidence, subjects were asked to use either a simple or elaborate "break-down exercise" to aid their judgment. Subjects were told:

> Becoming a regular user consists of 4 steps: awareness, trial, liking and switching. To become a regular user of olestra, a consumer must pass through all 4 of these steps.

In the elaborate decomposition task only, subjects read additional information explaining that "it is also possible to break down these steps into sub-steps… Sub-steps can include buyer and/or seller activities or other events." These subjects were asked to choose one of the four steps and break it down into sub-steps as an exercise. All subjects then carried out the decomposition task, by estimating the "percentage of people who become aware of olestra," "percentage of 'aware' people who decide to try olestra," "percentage of 'triers' who like olestra," and "percentage of 'likers' who switch to using olestra". Subjects were instructed on how to combine these numbers through multiplication to arrive at an estimate for "percentage regular users". Subjects were then told that "You may or may not wish to make adjustments to this number based on your own judgment" before providing a final estimate of % regular users in 2010. Subjects then indicated their confidence in this estimate. Finally, in the last set of questions, subjects rated their agreement with a series of statements concerning olestra and indicated their personal opinions about olestra, aspartame (NutraSweet), and their experience in the survey.

Results and Discussion

This study investigates whether an analogy influences judgment in a new product forecasting task and whether elaboration can neutralize the effect of an initial analogy. A main effect of the analogy on judgment would indicate that the analogy induced bias. Depending on when the dependent measure occurs, it is also possible to test for the effectiveness of elaboration (at time 1), presented evidence (at time 2), and decomposition (at time 3) in correcting for this bias.

Judgment After Elaboration and Presented Evidence

Results on most dependent variables support the hypothesis that the effect of an initial analogy will endure despite elaboration and presented evidence.[22] Subjects were asked to rate whether olestra is likely to succeed and how well it will sell (on 1-7 scales) both after initial elaboration and after presented evidence (see Table 19 and Figure 9). These attitude ratings were analyzed in a MANOVA with analogy and type of elaboration as between-subjects factors and repeated judgment as a within-subject factor. For judgments of *how well olestra will sell*, subjects who were given an initial positive analogy were more optimistic than subjects who were not given an analogy both after initial elaboration ($F_{1,91}$=3.85, p=.053) and after presented evidence ($F_{1,91}$=7.70, p=.007). The change in this rating from time 1 to time 2 was influenced by the type of elaboration ($F_{1,91}$=4.15, p=.019); nonetheless, the change failed to eliminate the effect of the initial analogy on final judgments.[23] Although there were no significant effects of the analogy on judgments of *whether olestra is likely to succeed* after elaboration or presented evidence (both F<1), the change in this rating from time 1 to time 2 was marginally more positive when subjects were given the positive analogy ($F_{1,91}$=3.16, p=.079).

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[22] CATMOD analysis of a binary judgment of success/failure taken after initial elaboration revealed no significant effects of analogy or type of elaboration on judgment. Neither did an ANOVA on confidence in this binary judgment (all non-significant). Notably, the majority of subjects (77 out of 98, or 78.6%) predicted success for olestra (77 out of 98, or 78.6%). The lack of significant findings on these dependent variables could be due to a ceiling effect because subjects are quite favorable toward olestra even in the no-analogy condition. These results are reported for completeness in Table 18.

[23] Because the type of initial elaboration (i.e., scenarios vs. reasons) does not appear to have consistent and interpretable effects on judgment across the dependent variables, this factor will not be discussed further.

Similarly, subjects estimated the percentage of regular users of olestra over time both after initial elaboration and after presented evidence (see Table 20). A MANOVA of these estimates with analogy and type of elaboration as between-subjects factors and estimation period and repeated judgment as within-subject factors revealed a significant main effect of the analogy on all of these estimates ($F_{1,93}$=6.40, p=.013).[24] Averaging across estimation periods, judgments were higher when subjects were given an analogy (versus not) both after elaboration ($F_{1,92}$=6.71, p=.011) and after presented evidence ($F_{1,92}$=5.42, p=.022). The change in judgments from time 1 to time 2 were influenced by an interaction of the analogy and type of elaboration ($F_{2,93}$=2.90, p=.060).[25] Nonetheless, this change failed to eliminate the effect of the initial analogy on judgment (see Figure 10).

Overall, these results on key dependent variables (success ratings and adoption estimates, both after elaboration and after presented evidence) support the hypothesis that the influence of an analogy on judgment is relatively enduring. As a further demonstration of this finding, an alternative analysis was conducted on these dependent variables.[26] First, a principal components factor analysis was performed, and three factors with eigenvalues greater than one emerged. Adoption estimates for the first two estimation periods loaded on the first factor (called 'early success'), success ratings loaded on the second factor (called 'overall success'), and adoption estimates for the last two estimation periods loaded on the third factor (called 'ultimate success'). These factor scores for each subject were then submitted to a MANOVA with analogy and type of elaboration as independent variables. The main effect of analogy was significant ($F_{1,91}=5.77$, $p=.018$): factor scores were higher for a positive (versus no) analogy, indicating more optimistic judgments by subjects presented with an analogy. Similarly, when a principal components factor analysis was restricted to one factor only, an ANOVA of this factor score for each subject revealed a significant main effect of analogy ($F_{1,90}=5.54$, $p=.021$). Thus, this alternative analysis also demonstrates a consistent effect of analogy that endures despite elaboration (scenario and/or reason generation) and presented evidence.

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[24] The MANOVA also revealed interactions of analogy and type of elaboration with estimation period (quadratic interaction: F2,93=2.45, p=.092; cubic interaction: F2,93=4.32, p=.016). These interactions reflect the notion that the effect of the analogy depends upon the type of elaboration and the time period. There is some indication that that the effect of the analogy is greater for scenarios than analogies but it does not appear to hold consistently across time periods. The possibility that scenarios exacerbate analogy-driven effects or that scenarios and analogies are mutually reinforcing is intriguing and merits further investigation.

[25] The interaction pattern is consistent but difficult to interpret. With no analogy, change is most negative in the reasons condition and least negative in the scenario+reasons condition. With an analogy, change is most negative in the scenario+reasons condition and least negative in the scenario condition. Again, an investigation of the interaction of type of elaboration with analogy merits future research.

Overall, these results suggest that elaboration (via a scenario and generated and presented reasons) fails to eliminate the effect of an initial analogy. Judgments continued to be influenced by the initial analogy after elaboration and presentation of evidence. Further evidence of the effects of the analogy on judgment can be found in subject ratings of the presented reasons (see Table 21). While positive ratings did not differ ($F<1$), negative ratings were marginally affected by an interaction of the analogy and type of elaboration ($F_{2,92}$=2.56, p=.0829).[27] Importantly, subjects given an initial analogy reported that the presented evidence made them more favorable and more confident about olestra's success compared to subjects given no analogy ($F_{1,92}$=3.58, p=.062). These self-report measures of attitude change (see Table 21) are indicative of subjects' mindsets and provide another indication of the continued influence of the analogy on judgment.

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[26] The details of these factor analyses are omitted due to space limitations.

Judgment after Decomposition

To analyze the effect of decomposition, I collapsed across the type of elaboration to avoid instability induced by small cell sizes.[28] The influence of the analogy is evident in subjects' estimates in the decomposition task. Recall that this decomposition task asked subjects to estimate the percentage of regular users of olestra in 2010 by breaking it down into component estimates for awareness, trial, liking and switching. A MANOVA of the four component estimates as a function of the analogy and type of decomposition reveals a significant overall effect of the analogy ($F_{1,95}$=3.75, p=.056). On average, component estimates are higher for subjects given an initial analogy, indicating that component estimates are susceptible to the analogy during decomposition (see Table 22).

Surprisingly, the effect of the analogy did *not* carry over to the calculation made by subjects based on these component estimates, although the effect of the analogy is in the expected direction ($F_{1,92}$=1.27, p=.263). However, a combined estimate calculated by computer does reveal the expected effect of the analogy ($F_{1,92}$=3.03, p=.085). Subjects' own calculations systematically erred in a positive direction, perhaps reflecting difficulty calculating small percentages or understanding conjunctive probabilities (or reflecting general optimism toward the new product). Interestingly, when subjects were given the opportunity to adjust their component-based estimate based on their own judgment, they did so, again in a systematically positive direction. Furthermore, their adjustments were influenced by the initial analogy and were more positive for subjects given an initial positive analogy ($F_{1,92}$=3.39, p=.069). Thus, the decomposition task proved to be ineffective at eliminating the effect of the initial analogy and was itself susceptible to the analogy's effects.[29]

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[27] However, the pattern of results does not seem easily interpretable.

[28] An additional analysis was conducted without collapsing across the factor representing type of elaboration. The pattern of results does not change significantly, although some higher-order interactions do occur. Because of the small cell sizes, the stability of these interactions seems questionable.

As further evidence of the persistent effect of the initial analogy and the failure of elaboration, presented evidence, and decomposition to eliminate its effects, I turn now to MANOVAs of some final attitudinal measures. Subjects were asked to rate their agreement with items regarding the success of olestra (see Table 23).

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[29] The effect of the decomposition task can also be tested by examining the *change* in judgment for the dependent variable measuring percentage regular users. Recall that subjects also estimated the percentage of regular users of olestra in 2010 after initial elaboration and after presented evidence (as the fourth estimation period in the adoption curve). A within-subject analysis of this repeated measure revealed an overall main effect of analogy ($F_{1,92}$=7.04, p=.0094), qualified by an interaction with the type of decomposition ($F_{1,92}$=4.97, p=.028). With no analogy, the elaborate (versus simple) form of decomposition appeared to induce greater change in judgment; with an analogy, the change was smaller. Thus, the decomposition task seemed to have greater impact in the absence of an analogy, perhaps because the breakdown task is itself susceptible to the initial analogy, thereby reducing its effectiveness. This possibility merits consideration and a further discussion of decomposition as a debiasing technique is reserved for Study 4. Nonetheless, as indicated by the overall main effect of analogy on these estimates, the change induced by the decomposition task failed to eliminate analogy-induced effects on judgment.

Subjects who were given an initial analogy were more likely to agree with these items than subjects not given an analogy ($F_{1,89}$=3.58, p=.062). These results can be interpreted as providing evidence of the analogy's effect on elaboration and inference making; that is, subjects appear to be reasoning by analogy to aspartame in thinking about the future of olestra.

Thus, the overall evidence from this study suggests that an analogy can have persistent effects on judgment. Under relatively conservative test conditions, neither elaboration (via scenario or reasons) nor presented evidence nor decomposition was sufficient to eliminate the effect of an initial analogy on judgment. The analogy continued to dominate judgment despite several attempts to induce evenhanded analytic thinking.

Table 18.
Pilot study 4: predictions of success and confidence in olestra as a function of analogy and type of elaboration.

Analogy	Type of Elaboration	# of Success Predictions	Confidence in Success
None	Reasons	14 / 18	68.33 (22.75)
None	Scenario + Reasons	10 / 16	63.13 (28.69)
None	Scenario	12 / 15	75.71 (19.89)
Positive	Reasons	16 / 18	73.06 (21.84)
Positive	Scenario + Reasons	12 / 15	70.67 (27.05)
Positive	Scenario	13 / 17	71.18 (25.71)

Note: Overall, 78% (77 out of 99) of predictions were for new product success.

Table 19.
Pilot study 4: attitude ratings toward olestra after elaboration and after presented evidence.

Analogy	Type of Elaboration	N	Attitude Rating (Unlikely/Likely to Succeed)		
			After elaboration	After evidence	Change
None	Reasons	17	4.94 (1.09)	4.71 (1.05)	-0.24 (0.44)
None	Scenario + Reasons	15	5.00 (1.60)	4.60 (1.59)	-0.40 (0.74)
None	Scenario	14	5.42 (1.34)	4.93 (1.59)	-0.50 (0.76)
Positive	Reasons	19	5.16 (1.42)	5.00 (1.45)	-0.16 (0.69)
Positive	Scenario + Reasons	15	5.00 (1.56)	4.93 (1.49)	-0.07 (1.16)
Positive	Scenario	17	4.88 (1.62)	4.88 (1.41)	-0.00 (1.06)
Main effect of analogy:			n.s.	n.s.	sig.
			Attitude Rating (Will Sell Poorly / Sell Well)		
			After elaboration	After evidence	Change
None	Reasons	17	4.82 (1.29)	4.59 (1.12)	-0.24 (0.56)
None	Scenario + Reasons	15	5.00 (1.36)	4.67 (1.40)	-0.33 (0.62)
None	Scenario	14	5.57 (1.45)	4.64 (1.15)	-0.93 (0.73)
Positive	Reasons	19	5.42 (1.07)	5.32 (1.00)	-0.11 (0.57)
Positive	Scenario + Reasons	15	5.80 (0.86)	5.47 (1.13)	-0.33 (1.18)
Positive	Scenario	17	5.65 (1.27)	5.12 (1.27)	-0.53 (1.01)
Main effect of analogy:			sig.	sig.	n.s.

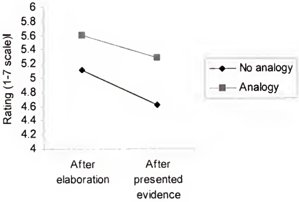

Figure 9.
Pilot study 4: attitude ratings (will sell very poorly/well) as a function of analogy after elaboration and after presented evidence.

Table 20.
Pilot study 4: adoption estimates for olestra after elaboration and after presented evidence.

Analogy	Type of Elaboration	N	Adoption Estimates (after elaboration)				
			at 1 year	at 2 years	at 5 years	at 10 years	Average
None	Reasons	18	39.06	44.53	52.22	56.33	48.03
			(27.85)	(24.20)	(25.30)	(31.11)	(23.27)
None	Scenario + Reasons	16	38.25	39.69	38.31	36.31	38.14
			(25.25)	(18.21)	(23.13)	(33.07)	(17.66)
None	Scenario	14	31.14	34.50	46.50	52.64	41.20
			(27.79)	(20.36)	(17.17)	(27.11)	(13.57)
Positive	Reasons	19	41.53	47.16	54.11	64.42	52.60
			(28.73)	(23.15)	(20.80)	(28.06)	(19.15)
Positive	Scenario + Reasons	15	39.00	45.27	55.00	63.67	50.73
			(29.53)	(23.15)	(17.79)	(23.10)	(16.79)
Positive	Scenario	17	50.00	56.59	54.82	54.65	54.01
			(27.10)	(23.33)	(26.22)	(37.63)	(20.55)
Effect of Analogy			n.s.	sig.	sig.	sig.	sig.
			Adoption Estimates (after presented evidence)				
			at 1 year	at 2 years	at 5 years	at 10 years	Average
None	Reasons	18	34.83	37.42	43.72	48.50	41.12
			(28.07)	(24.52)	(25.18)	(29.24)	(23.71)
None	Scenario + Reasons	16	38.25	39.81	38.69	38.19	38.73
			(26.54)	(22.13)	(23.80)	(32.01)	(21.90)
None	Scenario	14	28.86	29.93	35.64	43.57	34.50
			(24.65)	(20.36)	(18.50)	(26.32)	(15.72)
Positive	Reasons	19	37.05	42.47	49.37	58.95	47.69
			(29.25)	(23.00)	(23.68)	(30.03)	(21.15)
Positive	Scenario + Reasons	15	33.33	37.93	47.43	52.03	42.68
			(28.70)	(23.45)	(19.68)	(28.46)	(19.06)
Positive	Scenario	17	50.29	54.71	54.82	52.76	53.15
			(28.80)	(22.11)	(24.24)	(33.70)	(19.94)
Effect of Analogy			n.s.	sig.*	sig.	sig.	sig.

Note: * indicates that analogy X type of elaboration was also significant.

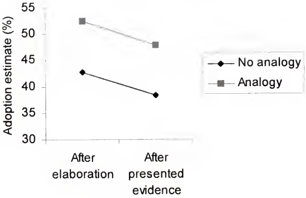

Figure 10.
Pilot study 4: average adoption estimate as a function of analogy after elaboration and after presented evidence.

Table 21.
Pilot study 4: ratings of presented evidence and change induced by evidence (self-reported).

Analogy	Type of Elaboration	N	Ratings of Positive Reasons	Ratings of Negative Reasons	Change in Attitude (Favorability toward Success)	Change in Attitude (Confidence in Success)
None	Reasons	16	10.39 (1.85)	10.61 (2.52)	4.75 (0.93)	4.63 (1.09)
None	Scenario + Reasons	16	10.00 (1.83)	8.75 (3.11)	5.13 (0.72)	5.06 (0.85)
None	Scenario	15	10.86 (2.11)	9.64 (2.59)	4.80 (1.47)	4.87 (1.60)
Positive	Reasons	19	10.68 (2.03)	8.89 (2.79)	5.37 (1.30)	5.42 (1.17)
Positive	Scenario + Reasons	15	10.71 (2.40)	9.21 (2.89)	5.73 (1.44)	5.53 (1.51)
Positive	Scenario	17	10.65 (2.37)	10.76 (2.41)	4.94 (1.48)	5.00 (1.50)

Table 22.
Pilot study 4: decomposition task estimates as a function of analogy and type of decomposition.

Analogy	Type of Decomposition	N	% awareness	% trial	% liking	% switch
None	Simple	25	76.40 (17.18)	58.20 (20.51)	47.28 (21.57)	41.64 (24.98)
None	Elaborate	24	72.71 (23.47)	55.42 (19.61)	38.96 (17.00)	35.17 (20.37)
Positive	Simple	21	77.05 (19.66)	61.76 (20.10)	54.57 (20.85)	47.19 (21.47)
Positive	Elaborate	29	75.72 (18.48)	60.79 (20.67)	56.10 (22.63)	46.93 (22.22)

			Algorithm output	Estimate calculated by subjects	Estimate adjusted by subjects
None	Simple	24	14.78 (17.98)	20.38 (22.22)	30.05 (21.29)
None	Elaborate	24	9.14 (11.89)	18.12 (19.31)	26.77 (17.91)
Positive	Simple	21	17.35 (17.59)	23.86 (21.16)	39.46 (24.29)
Positive	Elaborate	27	17.35 (12.63)	24.30 (20.91)	44.52 (25.04)

Note: Algorithm output is the product of the four component estimates.

Table 23.
Pilot study 4: agreement with statements favoring olestra as a function of analogy.

	No Analogy	Analogy
P&G should prominently display the Olean brand name on any foods that contain its brand of olestra.	5.41 (1.54)	5.85 (1.20)
Eventually, olestra will become as common as aspartame (better known by its brand name Nutrasweet).	4.76 (1.40)	5.26 (1.33)
I found it difficult to understand how olestra works in the human body. (omitted)	--	--
Olestra has many benefits.	4.91 (1.43)	5.00 (1.38)
Olestra has many risks. (reverse-coded)	3.50 (1.46)	3.47 (1.52)
The benefits of olestra outweigh the risks.	4.28 (1.38)	4.49 (1.44)
Comparing olestra to aspartame is (not) appropriate in judging olestra's future. (reverse-coded)	3.61 (1.68)	4.19 (1.60)
The future of olestra will be as successful as aspartame.	3.96 (1.58)	4.51 (1.20)
Average	4.35 (0.90)	4.68 (0.78)

CHAPTER 10
STUDY 3: ANALOGIES AND COUNTER-ANALOGIES

The pilot studies suggest that analogies have persistent effects on judgment. These pilots also indicate that elaboration (for example, generating *evenhanded* reasons) failed to eliminate the effects of an initial analogy on judgment. In light of these persistent effects of analogies, this study strengthens debiasing efforts by investigating whether *counterexplanation* and *counter*-analogies are sufficient to neutralize the effect of an initial analogy on judgment. It does so in a way that permits us to test whether analogies are differentially persistent to debiasing with counterexplanation, compared to one-sided explanation. Moreover, this study also offers direct evidence of "bias" by investigating whether consideration of multiple analogies is susceptible to order effects. In fulfilling these two objectives, this study will provide evidence regarding the potential effectiveness of multiple analogies as a management tool for improving forecasting. (Recall that, in a similar fashion, Study 1 examined the use of multiple scenarios in judgmental forecasting.)

Of course, order effects in reasoning are not new. Indeed, I have already reviewed prior research that demonstrates order effects in generating pro and con reasons (e.g., Hoch 1984). However, the results from the pilot studies suggest that analogies may dominate reasons. If so, *counterexplanation* may be less effective when it follows a one-sided analogy rather than one-sided reasons. Once a schema has been elaborated in support of an initial analogy, it may be difficult to undo this schema completely and then evenhandedly consider the opposite point of view. Based on this line of reasoning and my prior evidence, I predict that counterexplanation will be less effective at neutralizing the effect of an initial analogy (compared to initial reasons). In other words, analogies will be more resistant to debiasing with counterexplanation than reasons, thereby leading to stronger primacy effects in judgment.

The second objective of this study is to investigate the effectiveness of counter-analogies in eliminating the effects of an initial analogy. Interestingly, Einhorn and Hogarth (1987) recommend the use of alternative analogies to improve judgment. "Each metaphor illustrates a different factor, an alternative way of thinking. No metaphor by itself is adequate; considering them all provides a more complete picture" (p.68).[30] As an example, the authors consider how various metaphors illuminate different dimensions of a business school: finishing school (students mature before entering corporate life), military academy (preparing for economic warfare), monastery (receiving indoctrination in economic theology), diploma mill, job shop, etc. In other words, reasoning by analogy may unduly emphasize the importance of some dimensions of judgment at the expense of others. In contrast, an alternative analogy may break the conditional frame of reference established by the initial analogy (consistent with Koehler 1994) and re-frame and highlight different aspects of the judgment task. Hence, prompting people to consider multiple analogies may induce more evenhanded consideration of the relevant dimensions (a characteristic of analytic thinking). In addition, and consistent with the line of argument used by proponents of multiple explanation (Hirt & Markman 1995) and scenario analysis (Schoemaker 1993), considering alternative analogies may be sufficient to debias judgment.

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[30] Einhorn and Hogarth (1987) actually refer to metaphors. A metaphor suggests likeness between two objects directly so the authors' logic seems to extend to analogies.

Holyoak and Thagard (1995b) propose "analogy therapy" as "an antidote to fixation" but comment that the use of multiple analogies is not very well understood. I am unaware of any prior research that has investigated the use of multiple analogies to improve judgment. Research on reasoning by analogy suggests that people try to generate mappings that satisfy certain constraints (i.e., parallel constraint satisfaction) (Spellman & Holyoak 1992). A similar tendency may exist when people consider multiple analogies, that is, they may prefer the analogy that best fits the target. Holyoak and Simon (1999) suggest that both intra-analogy competition and inter-analogy competition may be similarly governed by constraint satisfaction. When the two analogies are approximately equal in terms of structural and surface similarity, people may prefer the analogy that best supports their goals (Spellman & Holyoak 1996). Such favoritism was evident in Holyoak and Simon's research on jury decision-making. Subjects who reached a verdict in a case shifted their assessments in favor of the analogy consistent with their verdict. Interestingly, the favored analogy continued to influence judgments in a subsequent case. This result was found when multiple analogies were presented *simultaneously.* Ordered presentation of analogies may exacerbate the effect if earlier reasoning by analogy and analogy-driven inference making serves to reinforce the initial analogy and make it more difficult for people to consider both analogies evenhandedly. Anecdotal evidence suggests that initial analogies can interfere with the acceptance of new analogies. For example, students typically first learn the Rutherford-Bohr model of the atom that uses an analogy to the solar system, but this orbiting concept conflicts with subsequent learning of models of the atom that incorporate quantum mechanics (see Holyoak & Thagard 1995b for an interesting discussion of analogies in science).

Thus, the use of multiple analogies in judgment is an open question. On the one hand, alternative analogies may break the conditional frame of reference induced by the initial analogy and prompt more evenhanded consideration of other dimensions of the problem. On the other hand, reasoning by analogy may drive elaboration that reinforces an initial analogy's effects. Once a schema has been elaborated in support of an initial analogy, it may be difficult to undo this schema completely and evenhandedly consider an alternative analogy. Based on this line of reasoning and my prior evidence, I predict that multiple analogies will be susceptible to order effects. That is, predictions will be biased in the direction of the initial analogy considered (primacy order effect). Considering counter-analogies will not debias judgment. Order effects would constitute direct evidence of "bias" and undermine the effectiveness of multiple analogies as an aid to judgmental forecasting.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of six cells in a 3 (Type of elaboration: analogy then counter-analogy, analogy then counter-reasons, reasons then counter-reasons) x 2 (Order: success then failure versus failure then success) between-subjects design. A total of 125 subjects participated, with the number in each cell ranging from 20 to 21.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. The survey asked subjects to play the role of a marketing manager and think about the future of vitalia-based snack foods at SnackTime. (Vitalia is a fictitious product ingredient.) Subjects were given some background information on vitalia and SnackTime as follows:

> Imagine that you are a marketing manager. You worked for a number of years in the new product division at a major beverage company. This experience has helped you to land a new job as head of new products at a snack food company, SnackTime Inc. This large company makes a variety of snack foods, including chips, cookies, crackers and so on. SnackTime's snack food products are very popular with consumers. SnackTime is facing an important decision concerning their products and has asked for your views as new product manager.
>
> The Research & Development Division at SnackTime has developed a new product, called vitalia™. When vitalia is added to the snack food formula during the manufacturing process, snack food flavors change and become stronger. In blind taste tests, consumers slightly preferred vitalia-formula snacks to regular-formula snacks. The research department is proposing that SnackTime switch to a vitalia formula for all of its snack foods.

> On the next few pages, you will be asked to consider SnackTime's situation and offer your views on whether SnackTime should offer vitalia-formula snack foods.

Subjects then considered reasons for and against the success of vitalia-formula snack foods. Some subjects also received an analogy manipulation (indicated in brackets in the following instructions). In their instructions, subjects were told to think about success as follows:

> Think about reasons why switching to vitalia-formula snack foods could be a success for SnackTime. (As you think about reasons for success, you may find it helpful to consider Coca-Cola's experience with the success of Diet Coke.) List as many possible reasons as you can why SnackTime should switch to vitalia-formula snack foods.

Subjects were asked to think about failure with the following instructions:

> Think about reasons why switching to vitalia-formula snack foods could be a failure for SnackTime. (As you think about reasons for failure, you may find it helpful to consider Coca-Cola's experience with the failure of New Coke.) List as many possible reasons as you can why SnackTime should not switch to vitalia-formula snack foods.

Both writing tasks were timed and lasted three minutes; between each task, subjects carried out a three-minute distractor task (listing five examples each of cities, colors, sports, universities, academic majors, movies, etc.) Subjects received the writing tasks in either order (success then failure or failure then success). Some subjects received no analogies (reasons then counter-reasons); some subjects received the initial analogy only (analogy then counter-reasons); some subjects received both analogies (analogy then counter-analogy). Subjects then answered a series of questions regarding their recommendation to switch to vitalia-formula snack foods, confidence in this recommendation, opinion toward vitalia-formula snack foods, and estimates of percentage of Americans consuming this new product over time.

Results and Discussion

The study predicted that judgments would be more biased toward an initial analogy than toward initial reasons generated. In other words, an analysis of the 2 (analogy versus reasons) x 2 (order) design is expected to reveal a significant two-way interaction, with a stronger primacy effect in the analogy-then-counter-reasons cells than the reasons-then-counter-reasons cells. In addition, a primacy order effect is also expected in the analogy-then-counter-analogy cells. No prediction is made regarding its strength relative to the other order effects at this time. Note also that an order effect in considering multiple analogies helps to rule out demand as an alternative explanation for these results. Results across the dependent measures are fairly consistent with these predictions.

First, consider subjects' recommendations regarding whether SnackTime should switch to vitalia-formula snack foods (see Table 24). An analysis indicates a marginally significant two-way interaction on switching recommendations (χ^2=2.96, p=.085). The order effect is significant in the analogy-then-counter-reasons cells (χ^2=9.93, p=.0016) but non-significant in the reasons-then-counter-reasons cells (χ^2=.73, p=.39). Moreover, there is an order effect in favor of the initial analogy when subjects considered multiple analogies (χ^2=7.41, p=.0065). An analysis of the two-way interaction for confidence in the switching recommendation is also significant and consistent with these results ($F_{1,119}$=9.17, p=.003). Subjects were more confident about switching when considering an initial positive analogy than an initial negative analogy followed by counter-reasons ($F_{1,119}=19.61$, $p=.0001$); the order of generating pro and con reasons had no effect on confidence ($F<1$). In addition, there was an order effect when subjects considered multiple analogies ($F_{1,119}=12.18$, $p=.0007$). Subjects were more confident when they considered a success analogy followed by a failure analogy, compared to failure then success analogies.

Second, subjects were asked to provide their opinion regarding the future of vitalia-formula products at SnackTime (see Table 25). Subjects rated the new product on four scales: "unlikely/likely to succeed," "will sell very poorly / very well," "not a good idea / a very good idea," and "do not/definitely recommend launch." A MANOVA revealed an overall two-way interaction ($F_{1,117}=6.27$, $p=.0136$). The primacy order effect was stronger in the analogy-then-counter-reasons cells ($F_{1,117}=15.56$, $p=.0001$) than the reasons-then-counter-reasons cells ($F<1$). In other words, subjects were more optimistic (pessimistic) when considering an initial positive (negative) analogy. Counter-reasons failed to eliminate completely the bias induced by an initial analogy, although it did eliminate the effect of initial reasons generated. Moreover, there was an overall primacy effect in favor of the initial analogy when subjects considered both analogies ($F_{1,117}=4.48$, $p=.0365$). In other words, counter-analogies failed to eliminate the effect of initial analogies. This pattern of means is depicted in Figure 11.

The same pattern of results was also predicted when subjects estimated the percentage of Americans regularly consuming vitalia-formula snack foods over time (see Table 26). Unexpectedly, these results failed to reach significance, perhaps masked by noise in the data. There was a main effect of order ($F_{1,119}$=8.75, p=.0037), but it was not qualified by an interaction with the consideration of analogies. However, the simple main effects are in the expected direction: the order effect in the analogy-then-counter-reasons cells is significant ($F_{1,119}$=5.70, p=.019) but only marginally significant in the reasons-then-counter-reasons cells ($F_{1,119}$=2.63, p=.1072). However, there was no order effect when subjects considered both analogies ($F_{1,119}$=1.03, p=.313), although the means are again directional (as depicted in Figure 12).

An analysis was also conducted on the reasons generated (see Table 27). Subjects were asked to *rate* the reasons that they generated for and against switching. Order influenced these ratings ($F_{1,118}$=4.42, p=.0377): subjects' ratings were higher for reasons in the first set (e.g., con reasons were rated as more persuasive and easier to generate when they were considered first rather than second). Subjects were also asked to *count* the reasons that they generated for and against switching. Order influenced the number of reasons counted ($F_{1,118}$=12.16, p=.007): number counts were higher for reasons in the second set (e.g., subjects counted more pro reasons when they were considered second). This pattern is interesting because the greater number of reasons in the second set was seemingly unable to compensate for the persuasiveness of the initial set of reasons generated. This contrast was particularly the case when subjects considered an initial analogy versus reasons ($F_{1,118}$=2.59, p=.110), suggesting that an initial analogy may lend persuasiveness to a set of generated reasons.

Overall, the results of this study provide considerable support for the notion that there are order effects in judgment for analogies and reasons. First, primacy order effects for analogies appear to be stronger than for reasons. An analogy leads to a 'stickier prior'; that is, the effect of an initial analogy is more persistent than the effect of initial reasons. Second, order effects endure despite counterfactual thinking. In contrast to prior research by Hoch (1984) on explanation and counterexplanation, counterfactual reasoning failed to eliminate the effect of an initial analogy. In addition, a counter-analogy failed to eliminate the effect of an initial analogy. This susceptibility to order effects raises questions about the efficacy of using multiple analogies to improve managerial judgment. Moreover, the consistent results in this study on analogies and in Study 1 on scenarios support the proposition that nonanalytic thinking has persistent effects on judgment that are difficult to eliminate with subsequent analytic (or nonanalytic) counterfactual thinking.

Table 24.
Study 3: recommendation to swtich to new snack food product and confidence in switching as a function of reasoning order and analogy cues (New Coke and Diet Coke).

Code	Type of Reasoning and Order 1st reasoning task	 2nd reasoning task	Recommendations To Switch	Confidence in Switching
A1	Positive Analogy + Pros	Negative Analogy + Cons	19 / 21	72.14 (13.03)
A2	Negative Analogy + Cons	Positive Analogy + Pros	10 / 21	50.00 (25.10)
AR1	Positive Analogy + Pros	Cons	19 / 21	68.81 (18.23)
AR2	Negative Analogy + Cons	Pros	8 / 21	40.71 (23.15)
R1	Pros	Cons	18 / 21	64.76 (18.54)
R2	Cons	Pros	15 / 20	64.00 (22.98)

Table 25.
Study 3: attitude ratings as a function of type of reasoning and order.

Code	N	Unlikely / Likely to Succeed	Will Sell Very Poorly / Very Well	Not a Good Idea / A Very Good Idea	Do Not / Definitely Recommend Launch	Average
A1	21	4.71 (1.23)	4.81 (1.21)	4.90 (1.73)	4.90 (1.26)	4.83 (1.26)
A2	21	4.29 (1.27)	4.10 (1.51)	4.05 (1.63)	3.95 (1.72)	4.10 (1.42)
AR1	20	4.95 (0.94)	4.85 (0.81)	5.20 (1.06)	5.00 (1.30)	5.00 (0.86)
AR2	21	3.81 (1.33)	3.76 (0.89)	3.38 (1.12)	3.48 (1.33)	3.61 (1.00)
R1	21	4.71 (1.01)	4.86 (1.06)	5.00 (1.26)	4.86 (1.11)	4.86 (0.97)
R2	19	4.47 (1.31)	4.74 (1.33)	5.00 (1.15)	4.68 (1.34)	4.72 (1.16)

Note: Code definitions (for type of reasoning and order) are described in Table 24.

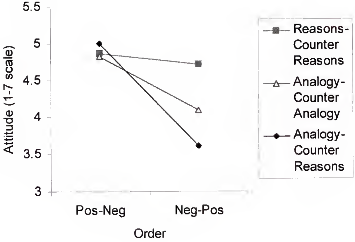

Figure 11.
Study 3: attitude toward new product as a function of type of reasoning and order.

Table 26.
Study 3: predictions of regular users of new product over time as a function of type of reasoning and order.

Code	N	Percentage regular users in next...				
		... year	... 2 years	... 5 years	... 10 years	Average
A1	21	25.00	33.10	38.67	42.05	34.70
		(21.67)	(22.40)	(24.17)	(25.73)	(18.83)
A2	21	21.05	28.90	31.00	34.14	28.77
		(18.56)	(21.13)	(20.45)	(25.81)	(17.53)
AR1	21	28.90	38.52	45.33	56.24	42.25
		(20.70)	(17.95)	(18.11)	(25.49)	(12.54)
AR2	21	28.10	27.10	29.05	33.88	29.53
		(23.74)	(14.10)	(17.38)	(25.24)	(13.89)
R1	21	25.62	36.43	49.19	59.90	42.79
		(20.54)	(20.73)	(23.36)	(28.90)	(20.40)
R2	20	25.25	31.13	37.15	42.60	34.03
		(18.07)	(17.26)	(21.13)	(29.13)	(19.05)

Note: Code definitions (for type of reasoning and order) are described in Table 24.

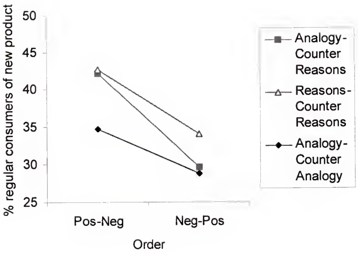

Figure 12.
Study 3: predictions of regular consumers of snacktime's new product (averaged over time) as a function of type of reasoning and order.

Table 27.
Study 3: self-ratings of reasons as a function of type of reasoning and order.

Code	Net reason ratings (ease and persuasiveness of pros minus cons)	Net number of reasons (pros minus cons)
A1	0.24 (1.19)	-0.48 (1.50)
A2	-0.48 (2.09)	0.00 (1.87)
AR1	0.10 (1.49)	-0.85 (1.81)
AR2	-0.50 (1.56)	0.81 (1.33)
R1	0.00 1.35)	-0.33 (1.39)
R2	-0.38 (1.01)	0.45 (1.32)

Note: Code definitions (for type of reasoning and order) are described in Table 24.

CHAPTER 11
STUDY 3A: ANALOGIES AND COUNTER-ANALOGIES IN A MARKETING SIMULATION ENVIRONMENT

The previous study demonstrated that counter-analogies and counterexplanation are insufficient to neutralize the effect of an initial analogy. This study is designed to address the use of multiple analogies in a relatively conservative test environment. First, analogies were again presented to subjects, thereby eliminating availability biases that could inhibit the generation of alternative analogies. Second, analogies were designed to be irrelevant, which should make it easier for subjects to discount or argue effectively against the analogies. Third, subjects were relatively experienced in the prediction task and were given additional relevant information (market research data), which should reduce the impact of less relevant information (such as analogies) on judgment. And, finally, the analogies did not include any explicit numerical information, thereby reducing anchoring as an alternative explanation for the results. Nonetheless, my hypotheses were consistent with those of Study 3, namely that the schema developed under an initial analogy is difficult to undo. That is, predictions are biased in the direction of the initial analogy (primacy order effect), and alternative analogies do not debias judgment.

Method

Subjects and Design

Subjects were undergraduate and a few EMBA students who participated as part of a class exercise. Each subject was randomly assigned to one of two cells in a 2 (Analogy: positive/negative) x 2 (Repeat predictions: after initial analogy and after counter analogy) mixed design. A total of 71 subjects participated, with the number in each cell ranging from 34 to 37.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. Subjects worked through the materials individually and at their own pace. The survey took the form of an exercise in BrandMaps (a marketing simulation game); all subjects were experienced players.[31] In BrandMaps, firms compete against each other in a "vaporware industry" (a fictitious product category used by the simulation game). Firms can market a varying number of brands in each region, and students are responsible for the launch, development and marketing of their firm's brands. Each region, firm and brand has a numerical designation (for example, firm 2-1 refers to firm #2 and its brand #1). Firms and regions are typically named in BrandMaps (although brands are not).

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[31] Students who participated in this study were experienced in the BrandMaps marketing simulation game, having played approximately 10 quarters. Thus, these students would have been familiar with the fundamental market drivers in the simulation and with the marketing research and financial information provided to them in BrandMaps. In this regard, use of the BrandMaps context is an attempt to examine judgment and decision-making with so-called 'experts' in BrandMaps.

The majority of the students were undergraduate seniors in a marketing management class. Approximately 10% were Executive MBAs in a marketing management class. There is insufficient power to statistically examine differences in judgment for undergraduates versus executives (as a proxy for novices versus experts). However, there are no directional indications that executives were any less biased than undergraduates.

It is also possible to examine judgment as a function of subjective (self-reported) expertise. Students were asked to rate themselves on several aspects of expertise. This scale was factor-analyzed and revealed two dimensions: BrandMaps expertise ('knowledge of decision variables', 'experience using BM research', contribution to my team's performance', 'attitude toward BM') and Cola market expertise ('knowledge of real-world Pepsi-Cola', 'knowledge of… New Coke', 'knowledge of… Diet Coke'). Students were categorized as high or low in subjective expertise on both of these factor scores. There is no evidence (directional or statistical) that subjective experts were less susceptible to bias than novices.

In the BrandMaps exercise, subjects were given background information about the competitors in a target region and their own firm's position in the region.

> Imagine that you are part of the management team for "Coca-Cola Vaporware" (Firm 1). You have just joined the firm and have been asked for your assessment of Coca-Cola Vaporware's position and future in the U.S. (Region 1). Summary statistics for Quarter 2 in the U.S. region are shown below. Briefly, you can see that Coca-Cola Vaporware faces 2 main competitors in its industry: 'Pepsi-Cola Vaporware' (Firm 2) and 'No-Name Vaporware' (Firm 3). Each of these competitor firms has one active brand in the U.S. Coca-Cola Vaporware also has one active brand 1-1 in the U.S., its flagship "Classic Coke" brand. Classic Coke is the leading brand in terms of earnings and ROI in the U.S.

Subjects were told that Coca-Cola Vaporware is considering the launch of a new brand as part of a project. The project name served as the manipulation of the initial analogy, either to an irrelevant past product failure or success. In the success analogy condition, students were told:

> Coca-Cola Vaporware has a project underway, code-named 'Diet Coke', to assess the feasibility of launching a second brand 1-2 in the U.S. Marketing research regarding the Diet Coke project--including competitor and customer information in Region 1, as well as specific research concerning Diet Coke--is attached (see Table of Contents). You may refer to this package of marketing research information at any time throughout this exercise.

In the failure analogy condition, the same instructions were provided with the label "New Coke" instead of "Diet Coke".

In both analogy conditions, students were given detailed information regarding the firm's marketing plan for the new brand (e.g., its formulation, marketing mix, etc.). Students also received the same firm summary statistics and marketing research (e.g. financial reports, concept and preference tests for the new brand, test market data, and other customer and competitor information). All students experienced in the simulation would be familiar with this information. Students were asked for a launch recommendation for the new brand, confidence in this recommendation, and predictions of market shares for the firms in the next quarter if the brand were launched.

Students were then asked to "briefly describe what you know about real-world Coca-Cola's experience with Diet Coke and New Coke in the soft drink market." After doing so, students were prompted to consider both the analogy and counter-analogy in an evenhanded manner as follows:

> You may or may not find real-world Coca-Cola's experience with New Coke and/or Diet Coke in the soft drink market useful in assessing Coca-Cola Vaporware's position in the BrandMaps vaporware market. Now that you have had a chance to give the matter some more thought, please answer the following questions related to the project launch described earlier.

Students then repeated their judgments for launch recommendation, confidence, and market share predictions. Students also answered various background questions about their expertise in BrandMaps and knowledge of cola markets.

Results and Discussion

Initial predictions were biased by the analogy to Diet Coke versus New Coke. Recommendations for launch were directionally biased in the direction of the initial analogy (χ^2=2.39, p=.12). Similarly, confidence in launch of the new product was marginally higher for subjects working on the Diet Coke versus New Coke project ($F_{1,69}$=3.18, p=.079). In addition, market share predictions were significantly different for the existing brand Classic Coke in the market ($F_{1,66}$=5.38, p=.023), but not for the new brand if launched (although directionally so). The differences appear to reflect the real-world reasoning that New Coke, compared to Diet Coke, may cannibalize sales of Classic Coke. This is evident in the direction of the means in Table 28 and was also mentioned in unanalyzed cognitive responses of subjects.

After students were prompted to think about *both* New Coke and Diet Coke, their *repeated* predictions continued to be biased by the initial analogy. The effect of the analogy on recommendations for launch and confidence in this decision are now both significant (respectively χ^2=3.57, p=.059; $F_{1,63}$=9.03, p=.004). Market share predictions for Classic Coke also continued to marginally differ by condition ($F_{1,55}$=2.93, p=.093); the difference for New versus Diet Coke remained non-significant. Overall, these results suggest that prompting consideration of both analogies did not eliminate the effects of the initial analogy.

Another view of the data considers the *change in responses* with repetition. In the intervening period between first and repeated judgments, students were asked to think about real-world Coke's experience with both Diet Coke and New Coke. This intervention serves as an attempt to debias judgment by prompting evenhanded consideration of both analogies. As we have already seen, the analogy does bias both initial and repeated judgments, so this attempt at debiasing was ineffective overall.

However, the question still remains: did the attempt at debiasing have at least some effect on improving subsequent judgment? The results appear mixed.

1. A MANOVA of both measures of confidence in launch of the new product as a function of condition and repetition indicates both a main effect of the initial analogy ($F_{1,63}=5.99$, $p=.017$) and an interaction with repetition ($F_{1,63}=6.33$, $p=.014$). As Figure 13 indicates, instructions to think about both analogies actually *exacerbates* the pre-existing bias, presumably by prompting additional analogy-driven reasoning beyond a mere anchoring effect.
2. A MANOVA of both Classic Coke market share predictions as a function of condition and repetition indicates only a main effect of the initial analogy ($F_{1,55}=3.52$, $p=.066$). This analysis does not provide any indication either of exacerbation or amelioration of judgmental bias. The biasing effect of the initial analogy *endures*.
3. A MANOVA of both New/Diet Coke market share predictions as a function of condition and repetition indicates no main effect of the initial analogy ($F<1$) but a marginal interaction with repetition ($F_{1,55}=2.98$, $p=.09$). This analysis suggests that any initial difference in predictions for New Coke versus Diet Coke is *reduced* after thinking about both analogies.

Overall, the data seem to suggest that the initial analogy continues to drive reasoning. Confidence in the launch decision drops for New Coke, but does not change for Diet Coke. Market share drops for New Coke, but not for Diet Coke; there are no changes in predictions for Classic Coke. These changes seem to reflect reasoning by analogy to the real-world experience of Coca-Cola, which suggests that New Coke will fail but Diet Coke will succeed. Interestingly, the major impact is via the analogy to New Coke, suggesting that spontaneous consideration of failure is otherwise unlikely, at least in this exercise. It must be said, however, that this is also a calibration issue that depends upon the favorability of the product information provided to students. If this information is sufficiently positive, there may be a ceiling effect limiting Diet Coke's influence on judgment. Because the design lacks a no-analogy condition, this issue cannot be addressed. However, the notion that prompting people to consider failure particularly improves judgment is intriguing and merits further investigation.

The findings of this study are consistent with the results for scenario analysis in Study 1. Just as counter-scenarios failed to eliminate scenario-induced bias, so did counter-analogies fail to eliminate bias due to an initial analogy. Test conditions were fairly conservative because the decision environment was rich in analytic information. In conjunction with Pilot Study 3, these results provide evidence that counterfactual reasoning techniques (generated reasons, presented evidence, and counter-analogies) are insufficient to debias judgment. In the next study, I continue to investigate the effects of analogies but employ decomposition as the debiasing technique.

Table 28.
Study 3A: new product launch decisions and forecasts after an initial positive/negative analogy and after a counter-analogy.

Condition (Initial Analogy)	Launch – Yes / No	Confidence in Go Decision	MS #s (%) New/Diet Coke	MS#s (%) Classic Coke
Initial Predictions (given an analogy)				
Positive Analogy	35 / 2	78.4	19.7	25.3
(Diet Coke)	(95%)	(16.8)	(12.5)	(9.1)
Negative Analogy	28 / 6	69.2	23.2	21.0
(New Coke)	(82%)	(26.0)	(15.7)	(5.4)
Final Predictions (given both analogies)				
Positive Analogy	30 / 3	76.7	17.2	25.4
(Diet Coke)	(91%)	(20.9)	(11.7)	(8.2)
Negative Analogy	23 / 9	58.0	17.0	22.3
(New Coke)	(72%)	(28.6)	(12.2)	(4.7)
	Change in % Launch	Change in Confidence in Go Decision	Change in MS #s (%) New/Diet Coke	Change in MS#s (%) Classic Coke
Positive Analogy	-4%	-0.76	-1.0	+0.7
(Diet Coke)		(13.8)	(5.5)	(3.5)
Negative Analogy	-10%	-12.3	-4.3	+0.9
(New Coke)		(22.5)*	(8.6) *	(2.8)

Note: * indicates change is significantly different from zero. However, the possibility of drift implies that contrasts to zero are an inappropriate analysis.

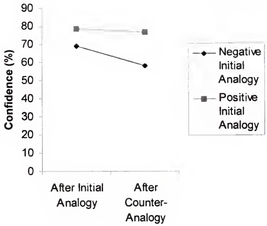

Figure 13.
Study 3A: confidence in new product decision after an initial positive/negative analogy and after a counter-analogy.

CHAPTER 12
STUDY 4: ANALOGIES AND DECOMPOSITION

Research on decomposition suggests that a 'divide-and-conquer' approach to problem-solving can lead to improved judgment. Quantitative estimation tasks have used decomposition to break down problems into sub-components that can be more easily and accurately estimated; these sub-components are then recombined into an overall estimation of the target quantity.

In a typical multiplicative decomposition, an algorithm is used to break down a target quantity into sub-components that, when multiplied together, provide an estimate of the target quantity. For example, Armstrong, Dennison, and Gordon (1975) asked subjects to predict the number of Polaroid color films used in the USA either through direct estimation or through decomposition algorithms provided by the experimenter (e.g., number of families in USA, percentage of families owning cameras, percentage using Polaroid cameras, average number of films packs used, percentage that were color). The authors found that decomposed estimates were more accurate for target quantities judged highly uncertain or difficult to estimate directly. Subsequent research has focused on two aspects of multiplicative decomposition: the types of target quantities that are helped by decomposition and the types of algorithms that are most effective. Research suggests that decomposition is especially helpful in quantitative estimation of extremely large uncertain quantities.

For example, decomposition improves accuracy for target quantities with extreme and uncertain values (e.g., auto accidents per year), but reduces accuracy for smaller values (e.g., US presidents) (MacGregor & Armstrong 1994). MacGregor, Lichtenstein, and Slovic (1988) also found that accuracy improved most in decomposition for problems in which the true quantity was extremely large because direct judgments grossly underestimate these quantities. More structured algorithms (e.g., algorithms including instructions on how to re-combine the component quantities) typically led to greater improvements in accuracy, as did experimenter-provided algorithms compared to subject-generated algorithms (see also MacGregor & Lichtenstein 1991).

Based on prior research, decompositional algorithms appear to be effective analytic tools that improve judgment compared to direct estimation. Looking across these studies, the main benefit of decompositional algorithms may arise from encouraging people to apply analytical reasoning to the prediction task.[32] Experimenter-provided algorithms improve accuracy because they cue relevant information and provide a structure for analyzing the problem. However, prior research has *not* investigated whether multiplicative decomposition is equally effective at improving judgment that is already biased by nonanalytic thinking. Nonanalytic thinking may create prior beliefs (e.g., an initial ballpark estimate) that reduce the benefits of decomposition, especially if bias creeps into each component of the algorithm and compounds error.

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[32] Other aids to managerial judgment may share this approach (e.g., decision analysis).

I have previously argued that reasoning by analogy may drive inference-making and lead to one-sided elaboration. Biased component estimates would provide evidence that inference making has been biased by an initial analogy. As a result, nonanalytic thinking that creates prior beliefs via top-down processing (e.g., relying on an analogy to provide an initial ballpark estimate) may reduce the benefits of decomposition by creating systematic bias in the decomposed estimates, which could carry over to the recombined estimates as well. On the other hand, a decompositional algorithm may prompt bottom-up processing that is less susceptible to the effects of an initial analogy.

This study serves as a relatively conservative test insofar as decompositional algorithms are provided to subjects by the experimenter. Debiasing may be particularly ineffective when people generate their own algorithms because the initial bias can influence both algorithm generation and estimation. For this reason, the decompositional algorithm was presented to subjects in an attempt to prompt relatively analytic thinking. The study is also designed to be conservative inasmuch as the product analogy is meant to be irrelevant. The analogous product, albeit from the same company, belongs to an entirely different product category (radon safety equipment versus child safety equipment). Surface similarity was manipulated through the name of the product in an attempt to alter perceived similarity. If this manipulation succeeds, then the initial analogy may be particularly influential when the products share other surface similarities, such as a similar name.

Method

Subjects and Design

Subjects were undergraduate students who participated for extra credit in an introductory marketing class. Each subject was randomly assigned to one of eight cells in a 2 (Analogy: to a product with high versus low sales) x 2 (Processing: bottom-up versus top-down) x 2 (Product name: similar versus dissimilar) between-subjects design. A total of 92 subjects participated, with the number in each cell ranging from 10 to 12.

Materials and Procedure

All experimental materials took the form of surveys distributed to each subject. Subjects worked through the materials at their own pace. The survey asked subjects to imagine that they were the new marketing manager for HomeAlert, a manufacturer of safety equipment targeted at the 60 million Americans who own their own homes. The survey then described RadonAlert, the first product manufactured by the firm, and its sales and market share (30 million unit sales and 50% market share in the high analogy condition; 1.2 million unit sales and 2% market share in the low analogy condition). The scenario then described a new product idea called ChildAlert in the similar name condition and ChildMinder in the dissimilar name condition.

In the top-down processing condition, subjects first gave an initial estimate of market share and unit sales of this product in the first 3 years; in the bottom-up condition, subjects proceeded directly to the decomposition task. The decomposition task asked subjects to estimate intermediate percentage probability estimates, which form the basis of a decompositional algorithm for estimating probability of success. Subjects then provided final estimates of market share and unit sales in the first three years, percentage probability that the product will succeed, recommendation to launch product or not, confidence in this decision, and ratings concerning the basis for the decision. A final open-ended thought-listing question asked subjects why this product might succeed or fail.

Results and Discussion

The rationale for this study predicts a three-way interaction between name, analogy, and processing on predictions for final unit sales, market share, probability of success, and decision to launch the new product, as well as on the intermediate probability estimates. An ANOVA indicated that this interaction was not significant for any of the dependent measures, with the exception of probability of success ($F_{1,83}=4.75$, $p=.032$) where the results do not appear to be easily interpretable or theoretically interesting (see Table 29 and Figure 14). Therefore, the surface similarity manipulation did not have the predicted effects.

The analysis now turns to the two-way interaction between analogy and processing on the dependent measures. The pattern of results from ANOVA on the continuous dependent measures reveals an interaction, although CATMOD and LOGIT indicated that this interaction was not significant for the binary decision to launch variable. The two-way interaction between analogy and processing was significant for final market share ($F_{1,79}=2.99$, $p=.088$) and unit sales ($F_{1,71}=5.82$, p=.018). Because of the significant three-way interaction with product name, separate analyses were conducted for probability of success in each product name condition. The simple two-way interaction was directional for probability of success in both conditions (dissimilar: $F_{1,83}$=2.42, p<.13; similar: $F_{1,83}$=2.33, p<.13).[33] As predicted, the analogy had no effect in bottom-up processing (all simple effects tests were non-significant) but had a significant effect in top-down processing for market share ($F_{1,79}$=6.09, p<.016), unit sales ($F_{1,71}$=14.37, p<.001) and probability of success in the dissimilar-names condition ($F_{1,83}$=10.88, p<.001) and similar-names condition ($F_{1,83}$=8.92, p<.004).[34] These results are shown in Tables 29-30 and Figures 14-15. Thus, in between-subjects comparisons, it is evident that subjects who provided an initial estimate were susceptible to analogy effects, and their judgment was not completely debiased by task decomposition. In contrast, subjects who did not provide an initial estimate but instead proceeded directly to a decompositional task did not appear susceptible to analogous product information. These between-subjects results are consistent with a top-down versus bottom-up processing view.

It is also possible to examine the effects of the decompositional task on a within-subject basis. An ANOVA of initial and final market share and unit sales predictions (measured before and after the decomposition task) indicates some change in subjects' predictions. For market share, the main effect of analogy ($F_{1,36}$=8.47, p=.006) is qualified by an interaction with repeat judgment ($F_{1,36}$=4.45, p=.042). For unit sales, there is only a main effect of the analogy ($F_{1,37}$=11.18, p=.002). Follow-up univariate analyses on this data indicate significant effects of the analogy on all estimates: initial market share ($F_{1,36}$=9.85, p=.003), initial sales ($F_{1,37}$=11.34, p=.002), final market share ($F_{1,36}$=6.78, p=.013) and final sales ($F_{1,37}$=10.41, p=.003). Thus, although repeat predictions are affected somewhat by the decomposition task, it does not completely eliminate the effects of the initial analogy.

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[33] Analysis of unit sales must be interpreted with caution due to variance heterogeneity. The analysis is consistent with ANOVAs on the other dependent variables.

[34] There were also effects of analogy X name ($F_{1,79}$=3.97, p<.050) and analogy ($F_{1,79}$=3.31, p=.073) on market share, and analogy ($F_{1,71}$=7.73, p<.007) and process ($F_{1,71}$=3.65,p=.060) on unit sales.

There are several reasons why the decomposition task may be ineffective at debiasing judgment. First, it must be noted that the decomposition task did not provide an explicit algorithm instructing subjects how to combine their intermediate estimates into an overall judgment (unlike Pilot Study 4). If subjects were not forced to reconcile their intermediate estimates with their final judgments and were free to judge and interpret the meaning of these intermediate estimates without any logical constraints, then some of the benefits of the decompositional task may be lost. Second, it is possible that the decompositional task itself is susceptible to analogy-driven effects. To investigate this possibility, a MANOVA was conducted on the intermediate estimates in the decomposition task. It revealed a marginal main effect of the analogy ($F_{1,74}$=3.13, p=.081), indicating that intermediate estimates tended to be higher in the high versus low analogy condition (see Table 31). If this is the case, quantitative estimates calculated from decompositional algorithms could also be biased and perhaps even exacerbate bias if systematic error is inflated due to the nature of the multiplicative algorithm (as was seen in the calculated estimates in Pilot Study 4).

Consistent with the notion that asking for an initial estimate induces strong anchoring effects, subjects were asked to rate the analogy's importance in their decision. An ANOVA on this self-reported measure revealed the expected main effect of process ($F_{1,81}$=7.44, p<.008). Subjects rated the analogy as more important in top-down processing than in bottom-up processing (see Table 32). There was also a significant main effect of product name ($F_{1,81}$=5.56, p<.021). Subjects rated the analogy as more important when the product names were similar than when they were dissimilar. These two effects suggest that perceived need or usefulness influences judgments of importance of information (Greenberg, Pyszczynski, Warner, & Bralow 1994) but that subjects may also be susceptible to irrelevant similarities such as product name. However, further research is needed to follow up on this possibility.

Overall, the most important finding in this experiment is the two-way interaction between the analogy and type of processing. As expected, asking subjects to provide initial market share and sales estimates led them to rely on analogous product information. The effect of the analogy endured despite a subsequent task that asked subjects to decompose their judgment into a sequence of conditional probability estimates. While task decomposition did have some corrective effect on judgment, it did not completely eliminate the effects of the initial analogy, as evident in comparisons to a bottom-up processing group that did not provide initial estimates. Two conclusions can be drawn from these results. First, immediate use of a decompositional task in quantitative estimation appears to induce bottom-up processing, thereby limiting the effects of analogies on judgment. Second, making an initial estimate appears to induce top-down processing with the analogy-driven estimate serving as a focal hypothesis. Once people adopt the analogy, the effectiveness of debiasing via decomposition is reduced. The first conclusion suggests a boundary condition on the influence of analogies; the second conclusion suggests a boundary condition on the effectiveness of task decomposition in improving judgment.

As I indicated previously, it is difficult to distinguish analogy-induced biases from anchoring effects when a numerical anchor is included with the analogy. Previous studies have examined analogy effects with and without explicit anchors. These phenomena also may be naturally confounded much of the time in the real world. If so, the results are still interesting whether seen as demonstrating analogy or anchoring effects. Prior studies also demonstrated that several analytic tools fail to eliminate nonanalytic biases in judgment. The partial effectiveness of decompositional techniques merits further investigation. On the one hand, decompositional techniques are influenced by reasoning by analogy or anchoring. On the other hand, decomposition appears to limit analogy or anchoring effects by inducing bottom-up processing.

Table 29.
Study 4: predictions for probability of success as a function of name, analogy and processing.

Name	Analogy	Processing	N	Probability of Success
Similar	Low	Top-down	11	43.64 (21.22)
Similar	Low	Bottom-up	11	55.00 (27.02)
Similar	High	Top-down	10	72.00 (26.69)
Similar	High	Bottom-up	12	63.33 (18.38)
Dissimilar	Low	Top-down	11	75.00 (16.43)
Dissimilar	Low	Bottom-up	12	57.25 (20.99)
Dissimilar	High	Top-down	12	45.08 (23.46)
Dissimilar	High	Bottom-up	12	47.08 (18.27)

Table 30.
Study 4: predictions of new product market share and unit sales as a function of analogy and processing.

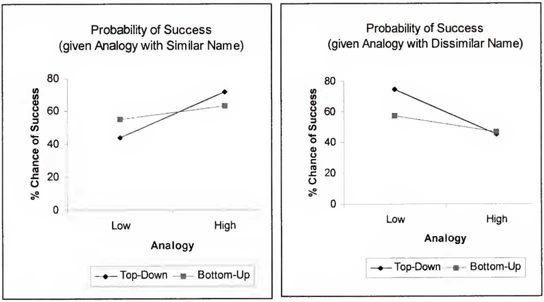
Analogy	Processing	N	Final Market Share (%)	N	Final Unit Sales (millions)	N	Initial Market Share (%)	N	Initial Unit Sales (millions)
High	Top-Down	21	44.24 (32.19)	22	17.15 (23.70)	20	44.86 (33.57)	21	16.374 (22.76)
High	Bottom-up	22	37.86 (32.43)	17	4.94 (7.21)		--		--
Low	Top-Down	21	21.45 (24.31)	20	2.29 (3.05)	22	20.33 (27.13)	22	1.19 (1.376)
Low	Bottom-up	23	37.17 (33.65)	20	3.75 (5.91)		--		--

Table 31.
Study 4: component estimates in the decomposition task as a function of analogy.

Component Estimates in the Decomposition Task (0-100% scale)	High Analogy	Low Analogy
Percentage of homeowners with children	60.0 (17.6)	63.3 (19.8)
Percentage of homeowners with children of the appropriate age group	36.6 (15.5)	38.0 (18.5)
Percentage of parents concerned about their child's whereabouts	77.2 (25.9)	71.5 (27.4)
Percentage of parents who would consider a monitoring system	53.5 (25.1)	45.6 (25.6)
Percentage of parents who would learn of the existence of ChildAlert/ChildMinder	51.0 (22.5)	50.5 (24.8)
Percentage probability that competitors will offer a similar product in the next 3 years	83.1 (18.7)	70.0 (28.9)
Percentage of consumers who would prefer ChildAlert/ChildMinder to similar competing products	60.7 (17.5)	53.0 (23.3)
Percentage of stores where these consumers shop who agree to carry ChildAlert/ChildMinder)	61.5 (25.5)	53.3 (28.7)
Average of probability estimates	60.5 (11.8)	55.7 (14.8)
N	37	45

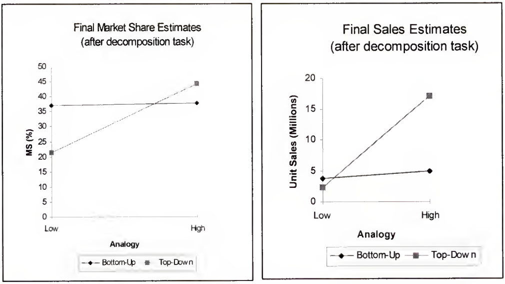
Table 32.
Study 4: importance rating of the analogy in the decision to launch new product.

Processing	Name	N	Rating
Top-Down	Similar	20	4.85 (1.82)
Top-Down	Dissimilar	22	3.82 (2.02)
Bottom-Up	Similar	23	3.67 (1.61)
Bottom-Up	Dissimilar	24	2.79 (1.93)



A B

Figure 14.
Study 4: predictions for probability of success as a function of analogy and processing in similar (A) and dissimilar (B) name conditions.



A B

Figure 15.
Study 4: market share (A) and unit sales (B) as a function of analogy and processing.

CHAPTER 13
GENERAL DISCUSSION

The studies reported in this dissertation provide evidence to support the main propositions of this dissertation.

P1: Nonanalytic thinking about scenarios and analogies has powerful effects on judgment.

P2: The effects of nonanalytic thinking are resistant to subsequent analytic thought.

The results of a set of scenario studies support these propositions. Pilot Studies 1 and 2 found that evenhanded reasoning does not eliminate the effects of imagining scenarios. Thus, the effects of scenarios merited further investigation. Study 1 demonstrated that a counter-scenario does not eliminate the effect of an initial scenario, thereby undermining the potential effectiveness of scenario analysis as an analytic tool for improving judgment. Study 2 provided evidence that accountability exacerbates, rather than reduces, the effects of scenarios on judgment, suggesting that real-world managers will not be immune to this nonanalytic bias. Turning to the set of analogy studies, Pilot Studies 3 and 4 demonstrated that deeper elaboration via scenarios and evenhanded reasoning does not eliminate the effects of an initial analogy. Thus, the effectiveness of other analytic tools following reasoning by analogy merited further investigation. Studies 3 and 3A demonstrated that counter-analogies fail to eliminate, but instead may exacerbate, the effect of an initial irrelevant analogy, thereby undermining the effectiveness of multiple analogies as a judgment tool. Study 4 provided evidence that decomposition fails to debias judgment that is already influenced by an initial analogy. However, decomposition does appear to reduce reliance on reasoning by analogy, although the decomposition task itself may be susceptible to the analogy's effect. Overall, these findings attest to robust effects of nonanalytic thinking on subsequent analytic judgment.

Stickier Priors

The most important finding of this research is that nonanalytic thinking leads to stickier priors. In Study 1, judgments were biased in favor of an initial scenario, and a counter-scenario was insufficient to eliminate its effect. Importantly, this primacy order effect was stronger for scenarios than reasons. Similarly, in Study 3, judgments were biased in favor of an initial analogy and counterexplanation was insufficient to eliminate its effect. Again, this primacy order effect was stronger for an initial analogy than for initial reasons only.

On the one hand, the basic finding of order effects in judgment is nothing new. Certainly, there is a great deal of evidence to suggest that people are non-Bayesian and experience difficulty in generating and evaluating multiple hypotheses, as I discussed in Chapters 2 and 5. My research takes this finding one step further, however, and provides evidence that analytic and nonanalytic thinking are *differentially susceptible* to order effects in judgment. People favor an initial scenario or analogy more than initial reasons. In other words, the initial scenario or analogy leads to greater fixation or a *stickier prior* than more analytic reasoning. But why is this the case?

As a rationale for this dissertation, I proposed that imagination of scenarios and reasoning by analogy are forms of elaboration that are under the guidance of a focal hypothesis. Such thought is unduly influenced by associations that are causally irrelevant (e.g., mental imagery in scenarios, superficial similarities in analogies). In addition, the focal hypothesis inhibits consideration of alternative hypotheses. As elaboration continues, a schema develops that supports the focal hypothesis and drives subsequent thinking. The schema drives inference making and leads to a well-integrated set of beliefs and inferences, in contrast to discretely encoded information. A scenario- or analogy-driven schema that is relatively coherent and consistent tends to be unduly convincing and influential; that is, the whole is greater than the sum of its parts. The enriched schema increases the availability and perceived plausibility of the focal hypothesis, thereby influencing subsequent judgment. This one-sided elaboration is a source of mental contamination that is difficult to undo. Although it was not the main purpose of this dissertation, I can nonetheless review evidence in my dissertation concerning this rationale.

Causally irrelevant information

I have previously alluded to imagination as a unique characteristic of scenarios. Pilot Study 1 offered evidence that, beyond mere explanation, imagination (i.e., causally irrelevant details and mental imagery) has powerful effects on judgment. Similarly, analogies may introduce causally irrelevant information (e.g., surface similarities). Although irrelevance is in the eyes of the beholder, Study 3A comes closest to demonstrating how a relatively subtle and irrelevant analogy cue can influence judgment despite the availability of considerable analytic information. On the other hand, my (possibly weak) attempt to manipulate the surface similarity of the analogy in study 4 failed and merits further investigation. Interestingly, the danger of causally irrelevant details may lie in their seeming irrelevance. Such details serve to embellish and fill out a schema and increase its perceived plausibility, yet their causal irrelevance may protect these same details from critical attention. For example, future research could examine whether prompting people to screen or assess analogies reduces the influence of irrelevant analogies. In addition, a more direct examination of the role of causally irrelevant information could be conducted by manipulating the degree to which scenarios contain mental imagery.

One-sided elaboration under a focal hypothesis

Both scenarios and analogies appear to drive one-sided elaboration under a focal hypothesis. Indeed, the notion of *elaboration* as a fundamental aspect of scenarios and analogies is supported by my research. Pilot Study 2 demonstrated that elaboration exacerbates scenario effects on judgment, and Pilot Studies 3 and 4 showed that analogies endure despite further elaboration. Scenario and analogy effects were also found on less direct judgment measures, which can be interpreted as evidence of inference making guided by the initial scenario or analogy. For example, scenarios influenced predictions of others' attitudes in Pilot Study 2 and Study 1 and launch recommendations for products and services in Study 1. Similarly, analogies influenced component estimates in the decomposition task in Pilot Study 4 and Study 4. There was also some (albeit weak) evidence that presentation or generation of evenhanded evidence may lead to further polarization (Study 1, Pilot Study 3 and 4). In other words, the effects of analogies and scenarios do not appear to arise from relatively superficial reasoning or a lack of thought. Overall, these studies provide evidence that elaboration contributes to scenario and analogy effects and its one-sidedness polarizes judgments. Future research about how people generate scenarios and reason by analogy is needed to understand what constitutes a good or compelling scenario or analogy and how it drives subsequent elaboration and judgment.

Resistance to debiasing

The persistence of nonanalytic thinking may lie in the elaborate and integrated schema that develops in support of the focal hypothesis. My research supports the proposition that the effects of scenarios and analogies are difficult to eliminate with subsequent analytic thinking. There may be many reasons for this. First, scenarios and analogies polarize judgment and thereby magnify the bias that must be neutralized with subsequent analytic thinking. For example, imagining success led to more optimistic judgments than explaining success in Pilot Study 1. Second, once judgment is polarized, it may bias subsequent analytic thinking and reduce its effectiveness. For example, decomposition was ineffective when judgment was already affected by an initial analogy in Study 4. Third, tightly integrated and elaborate schemas may be more difficult to undo than discretely encoded information (i.e., the whole is greater than the sum of its parts). My present work can offer no direct evidence on this point, although it is consistent with prior work on mental contamination and the findings of this dissertation. Finally, analytic reasoning may simply be ineffective at countering nonanalytic thinking. For example, Study 3 suggests that analogies remain persuasive despite counterfactual reasoning. What would be an effective counterargument for a convincing analogy or for vivid mental imagery in a scenario? My research suggests that analytic reasoning is not effective nor is fighting fire with fire by using counter-scenarios (Study 1) or counter-analogies (Study 3). Further research is necessary to fully understand the processes that contribute to the power of scenarios and analogies and their resistance to analytic thinking. For example, how does initial nonanalytic thinking affect subsequent analytic thinking, and how do people combine nonanalytic and analytic information in reaching a final judgment?

Ineffectiveness of Analytic Thinking

While this research has demonstrated powerful effects of scenarios and analogies on judgment, it has also shown the fallibility of various analytic tools heretofore thought to improve judgment. Traditional analytic thinking via evenhanded reasoning failed to neutralize scenario and analogy effects on judgment (in all pilot studies). Nor did counterfactual reasoning, counter-scenarios (Study 1) and counter-analogies (Study 3). The failure of these analytic tools does not appear to be entirely memory-mediated because presented evidence also failed to eliminate, and may even have exacerbated, nonanalytic effects on judgment. Presented information may provide grist for the mill and become integrated into the scenario's or analogy's schema. Similarly, initial nonanalytic thinking inhibited the effectiveness of decomposition (Pilot Study 4 and Study 4). Top-down processing driven by the initial analogy appears to dominate subsequent analytic techniques that strive to induce bottom-up processing. Accountability also did not confer immunity to nonanalytic thinking and seems to drive people to think harder, not smarter, and further elaborate in nonanalytic ways (Study 2). Thus, analytic tools are susceptible to nonanalytic thinking and therefore fallible, a finding that deserves emphasis for its contrast to prior research.

Construction in Judgment

In general, predicting the future reflects a process of *construction* rather than retrieval. One constructs the future, one cannot retrieve it from memory. It is interesting to note that much of the previous work on generating and evaluating multiple hypotheses has taken place in fairly analytic contexts that emphasize retrieval rather than construction processes (e.g., generating pro and con reasons; Hoch 1984). If differences between retrieval and construction are important, it may be possible to reconcile some contradictory findings in prior research. For example, the mere salience of alternatives or the breaking of a conditional frame of reference (Koehler 1994; Hirt & Markman 1995) may be sufficient to debias judgment when it is dominated by retrieval processes. One need only bring to mind other alternatives to obtain improvements in judgment. However, something beyond mere salience may be required to debias judgment that is based on construction processes. The act of construction may itself lead to a schema that is difficult to de-construct or undo, or that makes an alternative construction difficult to undertake. The uncertainty of a prediction environment makes it difficult for people to be deliberately evenhanded and analytic, thereby increasing reliance on nonanalytic thinking. Thus, the process of constructing the future seems especially susceptible to one-sided elaboration induced by a scenario or analogy.

In essence, a scenario tells a story about the future. In doing so, it helps people to organize and make sense of information. Similarly, an analogy also helps people to organize and make sense of information by providing a focal hypothesis that guides subsequent thinking. Very little may be needed to induce one-sided elaboration during the construction process (e.g., Holyoak & Simon 1999). Indeed, an initial leaning that develops in one direction or another may start people down the path of scenario generation and elaboration that ultimately leads to polarized judgment. Of course, other factors may also start elaboration in a particular direction. For example, an irrelevant analogy embedded in new product information may drive subsequent thinking and thereby polarize judgment. Similarly, situational factors may exacerbate one-sidedness in thinking (e.g., accountability and other incentives). Although there may be some motivation-based preference for coherent and consistent schemas and judgments, my research suggests that one-sidedness arises spontaneously as a result of the cognitive processes of scenario generation and reasoning by analogy.

Other Issues

Expertise

The studies described in this dissertation were conducted in a controlled laboratory environment using undergraduate students as subjects. Some readers may wonder whether, unlike students, managers would be immune to the potentially harmful effects of scenarios and analogies. Managers may differ from students on important characteristics. For example, managers have expertise and are held accountable for their judgments. These differences are presumed by some to confer immunity to error. My scenario studies suggest that accountability does not reduce susceptibility to error, so I will confine my remarks in this section to expertise.

First, new product innovation is an area in which expertise is difficult to develop. The failure rates of new product and businesses attest to this lack of expertise. By their very nature, new products are new and relatively unique and learning through experience is made difficult by the complexity and timeline of new product decisions. Amidst this difficulty and uncertainty, scenarios and analogies may be particularly seductive as a guide to judgment. Second, the effects of scenarios and analogies are attributable to psychological processes that expertise does not fundamentally alter. Prior research has demonstrated that managers are not immune to other judgment errors (e.g., the sunk cost effect; Boulding, Morgan, & Staelin 1997). It is difficult, then, to argue for immunity in the case of analogies and scenarios.

Two studies were designed to demonstrate the effects of scenarios and analogies with experts. In the first study (reported in Study 3A), both executive and undergraduate students gained experience in a marketing simulation game. Nonetheless, subjects' forecasts for the success of a new product were influenced by an initial irrelevant analogy. Neither their previous experience nor extensive analytic evidence provided in marketing research data nor a counter-analogy presented to subjects was sufficient to eliminate the effects of the initial analogy on their judgment. In a second study, MBA students who have completed an e-commerce course will be asked to use scenario analysis to forecast the future of a new Internet business venture. The purpose of this study is to demonstrate that initial scenarios dominate judgment even for these trained experts and even when counter-scenarios are generated. At the time of writing, this study was under preparation. Of course, experts may generate only a single success scenario, which may account for entrepreneurial optimism in the face of low base rates for success.

Groups

This dissertation focused on individual judgment. Some readers may wonder whether, unlike individuals, groups would be immune to the potentially harmful effects of scenarios and analogies. Interestingly, group research offers evidence consistent with the notion of one-sided elaboration and also suggests that groups will not be immune to these judgmental biases.

Group research points to the importance of *construction* processes in judgment and the danger of one-sided thinking. For example, both interpersonal influence and repeated expression of one's own attitude contribute to group polarization, which suggests that group polarization is basically a group explanation bias (Brauer, Judd, & Gliner 1995). Repeated expression was particularly powerful when expressed arguments were picked up and repeated by other group members, which is similar to exposure to one's own arguments again in presented evidence. In a similar vein, Heath and Gonzales (1995) propose that the key to understanding group-induced biases is whether the group interaction promotes or hinders "rationale construction" (whereby people organize and articulate their thoughts to support their opinion, a form of explanation bias). Predictions made by individuals after interactions with group members were no more accurate yet were more confident; similarly, a "directed writing" task also increased confidence whereas "pro/con writing" did not. These results are consistent with the notion that people prepare to support their opinion to the group and this explanation task biases subsequent judgment. Presented opposing arguments (i.e., counterarguing by others in the group) had little impact on individual judgment; however, actively generated pro and con reasons in a writing task did. Presumably, subjects who were instructed to engage in pro/con writing were encouraged to pay attention to and generate both supporting *and* opposing arguments, thereby preventing rationale construction and polarization of judgment.[35]

Rationale construction is reminiscent of story construction, whereby people construct stories to explain outcomes. Rationale construction may be a process similar to scenario generation and reasoning by analogy, inasmuch as it induces one-sided elaboration under a focal hypothesis, thereby increasing its plausibility and biasing judgment.

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[35] Research investigating dyadic interaction is also consistent with this notion. Sniezek and Henry (1989) found that groups were more confident and more accurate in estimating event frequencies (e.g., for causes of death), which the authors attribute to increased cognitive effort. Allwood and Granhag (1996) found that dyad members who answered almanac questions separately and then together exhibited lower overall overconfidence in their group versus individual answers. Confidence rose when pair members agreed and fell when pair members disagreed, suggesting that counter-explanation was at least somewhat effective (Allwood & Granhag 1996). The authors speculate on the importance of forcing subjects to utilize their knowledge and engage in discussion, thereby confronting counterarguments (in contrast to Allwood & Bjoerhag, 1990).

Managerial Implications

As new product innovation becomes increasingly important as a driver of sales and growth, it is critical for firms to identify successful new product ideas and forecast their future. The findings from this dissertation have a number of implications for managers who work on the development and introduction of new products. In particular, the early stages of thinking about new products are critical because, once judgments are formed, they are difficult to change. Judgments are particularly sticky when based on scenarios and analogies (compared to analytic reasoning). Thus, scenarios and analogies should be used with caution and subjected to greater scrutiny in new product forecasting, especially in the early stages of new product development. The use of analytic tools early in the forecasting process may reduce the inhibiting effects of nonanalytic thinking. At the very least, managers should reduce one-sidedness in judgment by using multiple counter-analogies and counter-scenarios, rather than allowing one specific analogy or scenario to dominate thinking. Future research could also investigate screening processes for the adoption of analogies and scenarios.

The findings from this dissertation also suggest that analytic tools may be less effective when they follow nonanalytic thinking. Evenhanded reasoning, presented evidence, counter-analogies, counter-scenarios, accountability and decomposition all failed to neutralize the effects of initial nonanalytic thinking. Once scenarios and analogies creep into the judgment process, they are difficult to eliminate with analytic thinking. Managers should beware of overconfidence in these (fallible) analytic tools. Moreover, managers can also assess organizational structure and processes for their susceptibility to nonanalytic thinking and modify the use of analytic tools to improve their effectiveness. For example, my dissertation suggests that managerial involvement in the generation of scenarios biases their subsequent evaluation. Disaggregation of these stages across managers may lead to improved forecasting. Similarly, changes in the product development process to separate nonanalytic thinking from subsequent analytic assessment should be examined. Although early design and development work may rely heavily on scenarios and analogies as a spur to creativity, their influence may be less beneficial in forecasting and launch decisions that require more analytic thinking.

In addition, these findings on scenarios and analogies may be applied to other areas of business and marketing, including planning and strategic decision-making. Plans share characteristics with scenarios: they describe in detail how the future will unfold and provide a blueprint for what the firm will do. As such, plans are susceptible to one-sidedness and will lead to sticky judgments that resist negative feedback and efforts to change them. This stickiness could account for the planning fallacy, a tendency toward optimism with plans (e.g., Buehler et al. 1994; Connolly & Dean 1997). For example, plans that fail to fully take into account all the things that can go wrong (i.e., worst-case scenarios) will underestimate new product development and launch times as well as needed resources. Strategic decisions, like new product decisions, are novel and complex and may rely on soft judgment guided by scenarios and analogies. For example, the merger of two firms is difficult to assess analytically, and analogies and scenarios for the success of a merger may be tantalizingly easy to generate. Witness recent upside scenarios and analogies to General Motors Corporation regarding the Time Warner-AOL merger (Business Week, 24 Jan 2000). The potential downside of nonanalytic thinking in strategic decision making and planning merits attention.

Consumer use of scenarios and analogies in responding to new product concepts, predicting their own behavioral intentions, and planning for their own futures, also merits further investigation. Scenarios and analogies may be powerful persuasive tools with which to sell new products to customers (or other constituents within or outside the firm). Indeed, Gregory and Duran (2000) discuss how consultants can use scenarios as a persuasive tool to increase client acceptance of forecasts and client commitment to strategic plans. Rather than telling customers why they should buy a new product, firms can tell them a story about how they will use a new product or provide them with a favorable analogy. For example, Hewlett-Packard has created a videotape scenario describing the wireless future of health care in hospitals, and new toys are often touted as the next Tickle-me-Elmo to persuade stores to stock them and customers to buy them. Once customers develop an initial judgment based on a scenario or analogy, this judgment may be fairly resistant to subsequent analytic thinking. Focus groups and concept testing can be designed to elicit the scenarios and analogies favored by customers for use in subsequent marketing efforts. The use of favorable analogies and scenarios may boost customer adoption rates for new products and encourage loyalty to, and reduce defection from, existing brands. In this way, firms may be able to take advantage of nonanalytic thinking.

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BIOGRAPHICAL SKETCH

Lisa E. Bolton was born in Toronto, Ontario, Canada. She obtained her B.Eng. from the Royal Military College of Canada in 1987, while serving as an officer in the Canadian forces. In 1990, Lisa completed her M.H.Sc. at the University of Toronto and then worked for five years as a clinical engineer at Mount Sinai Hospital and Women's College Hospital in Toronto, Canada. Lisa received her Ph.D. with a major in marketing from the University of Florida in 2000. Her research investigates judgment, prediction, and decision-making by managers and consumers, with substantive interests in new products, strategy, and consumer spending and finances. Her teaching background includes international marketing and consumer behavior, and she has done executive education at the University of Florida, Andersen Consulting and Intel. Lisa was appointed an assistant professor of marketing in the Wharton Business School at the University of Pennsylvania in 2000.

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

Joseph W. Alba, Chairman
Distinguished Professor of Marketing

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

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Christopher A. Janiszewski
Associate Professor of Marketing

I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

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Professor of Economics

This dissertation was submitted to the Graduate Faculty of the Department of Marketing in the College of Business Administration and to the Graduate School and was accepted as partial fulfillment of the requirements for the degree of Doctor of Philosophy.

May 2000

Dean, Graduate School

GENERAL AUDIENCE ABSTRACT

Judgmental forecasting is an inherent component of new product development and strategic decision-making. Managers and firms often struggle with these decisions, as evidenced by failure rates of new products and businesses. This dissertation investigates sources of bias in new product forecasting.

I identify two potential sources of bias: imagining scenarios and reasoning by analogy. In a series of laboratory studies, I demonstrate that an initial scenario or analogy has a persistent effect on judgment. Subsequent efforts to be more analytic (by generating reasons, or considering counter-scenarios or counter-analogies, or using other analytic tools) fail to neutralize the effect of a scenario or analogy.

In other words, a scenario or analogy creates a 'stickier prior' than a judgment based on traditional reasoning. Misleading scenarios or analogies may lead managers to make inaccurate forecasts. The persistence of these forecasts despite subsequent analytic thinking may account for some of the errors in new product forecasting.